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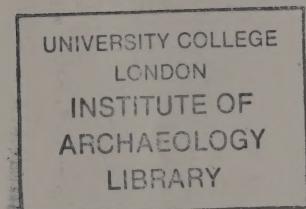
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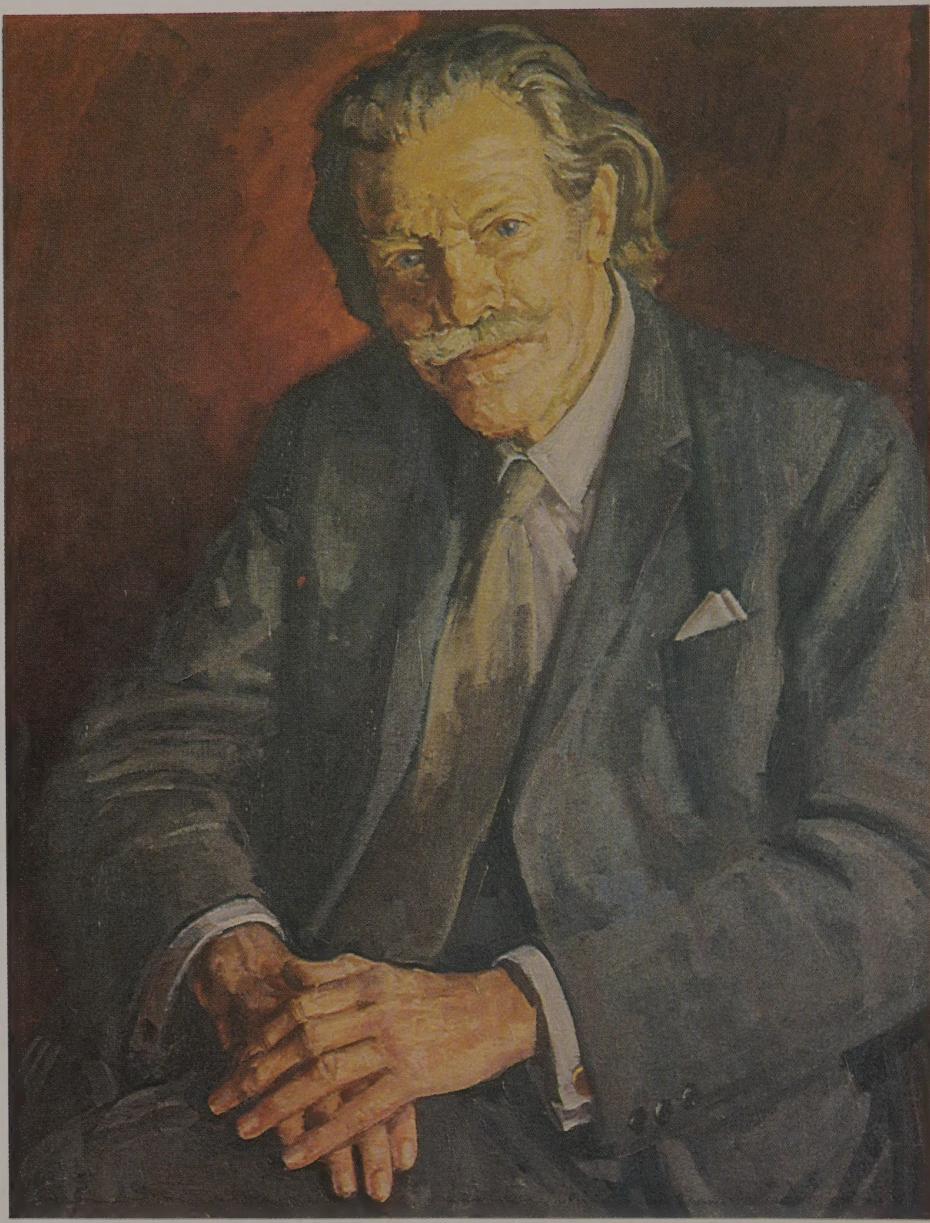


Plate I Sir Mortimer Wheeler (1890–1976), Honorary Director of the Institute of Archaeology from 1934–44 and Professor of the Archaeology of the Roman Provinces from 1948–55.

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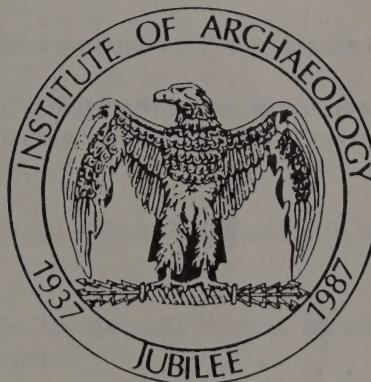
INSTITUTE OF ARCHAEOLOGY

GOLDEN JUBILEE

BULLETIN

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The First Half-Century – and After

by JOHN D. EVANS
Director

On 29 April 1937 a distinguished company of people assembled at the newly refurbished mansion of St John's Lodge in the Inner Circle of Regent's Park for its formal opening by the Chancellor of the University of London, the Earl of Athlone, as the home of the University's Institute of Archaeology (Plate 1). It is the fiftieth anniversary of this event that we are celebrating this year; but the Institute had in fact been established by the University three years earlier in 1934, and admitted its first two students, Rachel Clay (later Mrs Maxwell-Hyslop) and Barbara Parker (now Lady Mallowan). For three years it had been a 'paper Institute' only and the acquisition of a building was so crucial for its long term viability that it seems reasonable to take the opening of St John's Lodge as marking its definitive inauguration.

The Institute of Archaeology owes its existence chiefly to one man, or, more correctly, to one man and his wife, namely Dr R. E. M. Wheeler, as he then was (Plate I) and his first wife Tessa. The concept was his, but much of the credit for the hard grind needed to turn his dream into a reality belonged to her. They were a formidable team: he, brilliant, ambitious and endowed with an irresistible drive and energy which were truly demonic (in all senses of the term); she, the perfect foil to this, able, caring, tactful, responsible and an excellent organiser. Wheeler incubated the idea, as he tells us in his autobiography, during the early 1920s, when he was Keeper of Archaeology and later Director of the National Museum of Wales. But early in 1926, when he was 32 years old, he was offered the post of Director of the London Museum and accepted, partly because 'London was the only possible milieu for such a project'.¹ Since it was to be, in his own later words, 'a full dress academic institution', that meant London University; and since he was himself a graduate of University College, and had later been elected a Fellow, it is hardly surprising that he saw his connection with the College as the best means of forwarding his project in the University. As he characteristically phrased it 'the University of London, through University College . . . had to be led gently into the garden and up the right path'.² Thus, all unknowingly, the College became linked with the genesis of the Institute.

Archaeology was at that time by no means a new subject either to the College or to the University. By 1926 some branches of archaeology had been taught in London for several decades. The Yates Chair of Classical Archaeology was



Plate 1 The opening ceremony at St John's Lodge, Regent's Park, on 29 April 1937. *Left to right:* S. J. Worsley (Acting Principal of the University); Dr R. E. M. Wheeler; The Earl of Athlone (Chancellor); H. Lightfoot Eason (Vice-Chancellor); The Rt Hon. W. Ormsby Gore (later Lord Harlech) (Secretary of State for the Colonies and formerly First Secretary of Works).

established at University College in 1885. The Edwards Chair of Egyptology followed a few years later in 1892, again based at University College. A Board of Studies in Archaeology came into existence as early as 1903. Only two members of that first Board fell into the category of 'Teachers of Archaeology in the University'; they were Ernest Gardner, the Yates Professor, and Flinders Petrie, the Edwards Professor. The rest were 'Teachers in Cognate Subjects', and 'Other Persons', but the 'Other Persons' category included Dr A. J. Evans (later Sir Arthur Evans of Minoan fame).³ By 1906 the Senate had accepted Archaeology as a subject for both the BA and MA Honours degrees and gave a formidable list of options.⁴ I do not know how much of this ambitious scheme was implemented. Wheeler was perhaps unduly dismissive in referring contemptuously to 'occasional Egyptology and a little Classical art-history', but certainly none of it even approximated to the kind of archaeological training he was concerned to establish. It was academic archaeology in the traditional sense, while his proposed institution was intended primarily to

provide training in fieldwork techniques and the ancillary skills needed for the proper recording and publication of the results of fieldwork. ‘The next advance in our knowledge of human achievement’, he wrote several decades later, ‘was dependent on fresh and methodical discovery, and . . . fresh discovery meant fresh digging’.⁵ To achieve its purpose, however, such digging would have to be technically much better than all, or almost all, of what had been done up to that time and would need to be better recorded and better published. During his years at the National Museum of Wales Wheeler had begun to develop his own methods of excavation and recording and ‘it became a mission . . . to gather the younger generation about me in all my fieldwork, to inculcate it with controlled enthusiasm, and to give it in the formative stage a sense of direction, or at least the *need* for direction’ (Plate 2).⁶

Some months after his arrival in London, at the beginning of 1927, Wheeler presented a proposal to the University Board of Studies in Archaeology, not for an Institute, but for what he called a School of British Archaeology.⁷ This was to be comparable with the various British Schools of Archaeology abroad, such as Rome and Athens, in co-ordinating archaeological research, but it was also to incorporate a strong teaching element based on a two-year Diploma course which would include field and technical training as well as ‘the outlines of Western European archaeology, with special reference to Britain, from Palaeolithic times to the 17th century’. This School should be based in London and ‘should be closely associated with a



Plate 2 Wheeler relaxing with students on a dig.

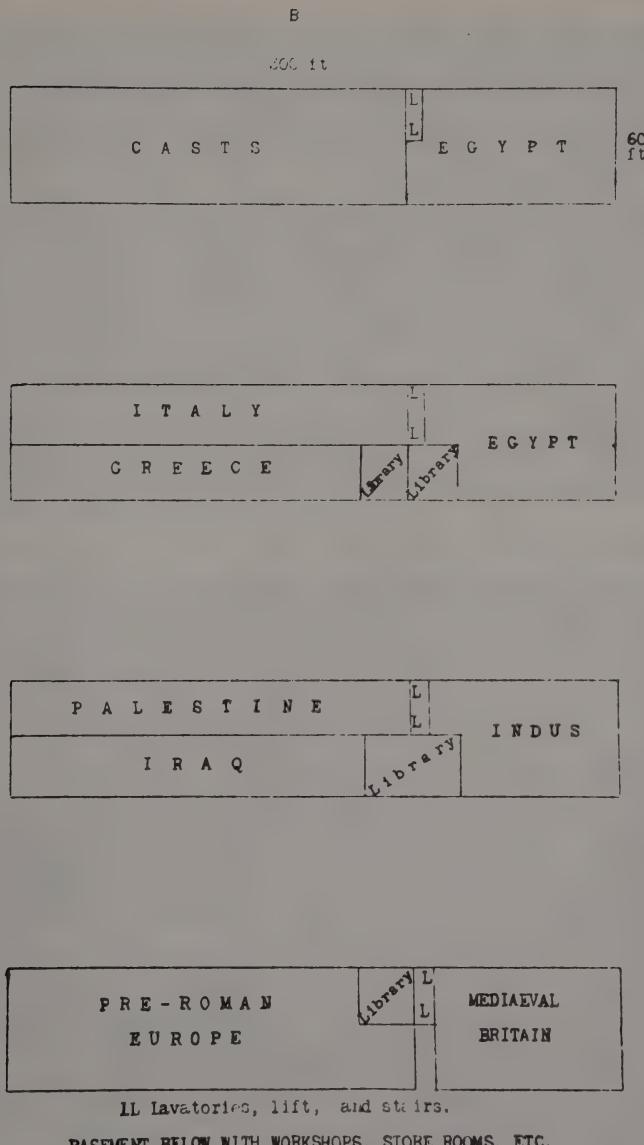
University curriculum'. 'It is possible', he went on, 'that the best solution would be to make the School an integral part of the University of London'; adding diplomatically 'but this is a matter for further consideration'.

The Board of Studies welcomed Wheeler's proposal warmly, but then proceeded to 'gallop off madly in all directions' with it. What eventually reached the University the following year was a much more grandiose scheme for a 'comprehensive institute of archaeology where students from all parts of the world shall find adequate facilities for training and research'.⁸ With the geographical limitation the emphasis on field and technical training was also dropped, to be replaced by stress on the large teaching collections from many parts of the world, a comprehensive library and 'indexes covering the whole field of archaeology'. The leading spirit in this transmogrification of Wheeler's proposal was Sir Flinders Petrie, an old friend of his, and, according to his lights, an ally. In his autobiography Wheeler implies that Petrie's schemes were just an old man's dreams and fails to mention that the Board of Studies was involved at all.⁹ In fact, the Board evidently discussed the whole matter very thoroughly and it produced a well-reasoned memorandum arguing the need for such an Institute and the unique advantages of the Bloomsbury site, then just adopted as the University centre, as a location for it.

Negotiations with the University over the Board's proposals went on for the next four years, during which they underwent various modifications. In their initial form they were for a four-storey building, plus some basement space, comprising 72,000 square feet in all (Plate 3). This was later scaled down to 30,000 square feet, to be provided by building an attic storey on top of part of the University building.¹⁰ This, it was felt, would have the double advantage of providing top lighting and being cheaper to build. It would also be stepped back so as not to be visible from the ground.

The University made sympathetic noises. The idea of Institutes was rather in vogue at that period. The Institute of Historical Research had been established several years earlier; the Courtauld Institute of Art was founded in 1932. There was talk for a time of an Institute of Anthropology. All of them would interrelate and, if located in close proximity on the new site, would be able to co-operate easily. It was a heady prospect. In this atmosphere University College was even persuaded to agree to the transfer of the Egyptology and Classical Archaeology collections to the new Institute of Archaeology. But it was all along made clear that if the University were to provide the space, the funds would have to come from sources other than central University money, and it was on this rock that the Board of Studies schemes eventually foundered.

Meanwhile, Wheeler was consolidating his position at University College and already implementing some of his ideas by using the London Museum as a base of operations. From 1928 the College provided him with a titular Lectureship in Prehistoric Archaeology which, though unpaid and unestablished, gave him the opportunity of attracting a few students, but it was at Lancaster House, where the



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Plate 3 One of the two alternative plans for an Institute of Archaeology in the Board of Studies' 1928 Memorandum to the University. (University of London Library, Archives of the Central Offices, CF1/29/663.)

London Museum was located, that, in his own words 'pending the establishment of the Institute of Archaeology of my dreams, the shadow of a research institute was already coming into being'.¹¹ His university lectures were also mostly given in the congenial atmosphere of the museum, and research students came to work there. In 1929 the College made his titular post into a regular part-time Lectureship, and a postgraduate diploma was inaugurated.

By 1930 he had also become Secretary of the Board of Studies in Archaeology and, when the Board's proposals were clearly running out of steam a couple of years later, he was ready to take the initiative. By the beginning of 1932 he had steered the Board towards the idea of setting up an Appeal Committee, chaired by the President of the Society of Antiquaries, Sir Charles Peers, to raise funds for a still further reduced scheme, which eventually, after considerable discussion, excluded the Egyptian and Classical collections and required only 11,200 square feet of space.¹² During the next few months a powerful Appeal Committee was brought into being which included distinguished representatives of the main archaeological institutions in the country. Tessa Wheeler became the Secretary and the Appeal was duly



Plate 4 The front of St John's Lodge in 1937.

launched in the early part of 1932. But despite favourable auguries, and much hard work, by early in 1934 less than £7,000 had been collected. A further proposal was therefore submitted to the University by the Board, though evidently master-minded by Wheeler, for the immediate establishment of an Institute of Archaeology on a ‘much more economical scale’.¹³ There was now no question of affording the premises in the new University building. Much cheaper accommodation had to be sought, a task which fell largely on Tessa Wheeler. One possibility which was very seriously considered was an offer from University College of a fourteen year lease at a nominal rent on space in one of its recently acquired properties north of Malet Place.¹⁴ This was the first floor of Block B, a former stable, in Foster Court, which now houses the Department of Egyptology and the Petrie Museum. Though scarcely adequate even for the Institute’s reduced needs, nor offering any room for expansion, it was at least in Bloomsbury and was something that might be afforded even with the meagre funds available, especially as the College generously offered extended credit for the costs of conversion. Nevertheless, the exhausting search went on for something better, but also ‘at next to no rent’.¹⁵

In the meantime the Senate finally approved the establishment of the Institute in accordance with the latest proposals.¹⁶ On 4 July 1934, a number of members of the Appeal Committee were summoned to a meeting ‘to constitute the administrative body of the Institute’.¹⁷ They were asked to draw up a list of names for a Management Committee for submission to the Senate; to make a recommendation to the Senate regarding the post of Director, and to appoint a Secretary. Wheeler was, naturally, recommended for Director and Tessa appointed Secretary (both Honorary posts, of course). At this stage it seems still to have been fully expected that the Malet Place site would be the Institute’s home. Three weeks later the first meeting of the new Management Committee took place, but by then the situation had changed radically.¹⁸

First, a letter from the Office of Works was read to the meeting in which a tentative offer was made of St John’s Lodge (Plate 4) for the Institute on a fourteen year lease at a nominal rent if the Institute would recondition it and undertake to maintain it to a satisfactory standard during the tenancy. ‘It was exactly what we wanted’, wrote Wheeler in *Still Digging*. ‘It already possessed two libraries equipped with shelving; a vast ballroom offered lodging to the sprawling but invaluable Petrie Collection; an annexe could serve conveniently for some time as a laboratory; classrooms and studies were there in plenty for some time to come’¹⁹ (Plates 5–7). The Management Committee enthusiastically endorsed the recommendation which he presented to them.

Reference was then made for the first time to a potential anonymous benefactor, who ‘had signified her desire to assist financially’. This later turned out to be a lady who had offered Petrie £10,000 to help provide a permanent home for his Palestinian collection. Petrie, now over eighty and about to leave England for good to live in Palestine, had persuaded this lady (who was revealed, after her death a few



Plate 5 Petrie's Palestine material awaiting unpacking at St John's Lodge, 1936.

years later, as a Mrs Mary Woodgate Wharrie) to transfer her offer to the new Institute of Archaeology, provided that body would accept responsibility for his Palestinian material. In *Still Digging* Wheeler implies that Petrie had offered to arrange this for him even before the formation of the Appeal Committee in 1932.²⁰ If so (and it may well be so) the offer seems for some reason to have been kept very dark during the intervening two years. The letter from the Office of Works which appeared so pat at the first Management Committee meeting also implies a good deal of behind the scenes work beforehand.

The prospects for the Institute were now greatly improved in a number of ways. St John's Lodge, though somewhat remote from the University centre, clearly offered much better accommodation than the Malet Place building and Mrs Wharrie's benefaction, which was soon confirmed by her solicitors on satisfactory terms, made the venture just possible financially. The Appeal was to be kept going and there were lively hopes for further donations. In the event, by far the largest of these to materialise was a further £5,000 given in two stages by Mrs Wharrie, who thus became the Institute's greatest, though to some extent unintentional, benefactor.²¹



Plate 6 The Petrie Collection as laid out in the former Ballroom at St John's Lodge.

The Institute was now neither quite Wheeler's original 'School of British Archaeology' nor the 'comprehensive institute' envisaged by the Board of Studies, but a slightly uneasy compromise imposed by economic realities and University politics. The Egyptian and Classical collections were to remain in University College, but the proposed British collections were to be joined immediately by Petrie's Palestinian material and soon by other material as well. It had always been accepted by all parties that the Institute of Archaeology was to operate more or less along the lines of the Institute of Historical Research, providing first and foremost a centre for study and research in the subject and fulfilling only a co-ordinating role in teaching, using teachers based in the Schools and Colleges. However, it was recognised that since there were no posts in many important branches of archaeology in any of the Schools, there would have to be a 'transitional period',²² during which arrangements would have to be made for teaching at least some of these at the Institute. British archaeology was already taught by Wheeler in his capacity of part-time lecturer at University College and so was not included. It was now obvious, however, that Palestinian archaeology must be taught at the Institute; and at the first meeting of the



Plate 7 One of the Libraries at St John's Lodge.

Management Committee Wheeler, as Director, abruptly introduced a proposal for a course in Mesopotamian archaeology.²³ This he linked to a projected Department of Near Eastern Archaeology with various sub-departments, including Palestine; but it seems likely that the immediate stimulus for its introduction at that stage was the interest expressed in it by the two prospective Diploma students, Rachel Clay and Barbara Parker.

Sidney Smith of the British Museum and Professor Hooke of King's College accepted Honorary Lectureships at the Institute and agreed to provide the Mesopotamian course and other Near Eastern teaching.²⁴ Later, Kathleen Kenyon, who was appointed paid Secretary of the Institute in 1925 (at £200 per annum),²⁵ also took on the teaching of Palestinian archaeology. For Wheeler undoubtedly the core of the whole enterprise continued to be centred on field and technical training rather than any regional teaching; this was still firmly linked in his mind with his own work in Britain. It became clear, however, that the Director of the Institute should at least have seen something of what was going forward in the Near East. Accordingly, in the early part of 1936, Wheeler made a six-week tour of Near Eastern excavations which

brought him face to face with shortcomings in excavation and recording techniques which turned out to be even worse, to his mind, than anything in Britain. He returned with broadened horizons, 'ferociously determined to make my new institute in London first and foremost an effective medium for the enlargement of technical understanding'.²⁶

Despite Tessa Wheeler's tragic death just before his return from this trip, preparations for the Institute continued. The apparently interminable negotiations over the lease at last reached a termination and the rehabilitation of the building began. On 12 October 1936, the Institute finally entered into occupation and the unpacking of the Petrie collection began. Wheeler's original scheme for a 'School of British Archaeology' had envisaged, in addition to provision for field training and instruction in drawing and surveying, a 'repair workshop', which could also give some instruction in 'the preservation of antiquities'. This desideratum was now provided for by two young ladies, Delia Parker and Ione Gedye, who had already



Plate 8 Ione Gedye and Delia Parker at work.

been operating for some time at the London Museum (Plate 8). Here then were the humble beginnings of our present Department of Conservation and Materials Science. More interestingly still, Wheeler's 'British School' document of 1927 calls for courses in what we would now call environmental archaeology: 'geology, especially pleistocene and recent; physiography and climate; anatomy, human and animal, the latter with reference to pleistocene and recent fauna'. Nothing further had been explicitly mentioned about all this in any of the proposals made since then, but the need was now supplied at the right moment in a way which must have seemed like manna from heaven. In November 1936 Wheeler reported to the Management Committee that 'he had been consulted by the Academic Assistance Council' (an organisation formed to help the resettlement of refugee academics from Europe) 'in regard to the future of Dr F. E. Zeuner, late Professor of Geology and Palaeontology at Breslau, who had been dismissed since his wife was a "non-Aryan"'²⁷ (Plate 9). The Council was prepared to continue to support him for three years; it was therefore agreed that he should be appointed Honorary Lecturer in Geochronology



Plate 9 Frederick Zeuner and Gordon Childe.

at the Institute for that period and provided with a room there. Thus began our Department of Human Environment and, with it, what was perhaps even more significant for the future, our tradition of including scientists on the staff of the Institute.

For a little over two years after the grand opening ceremony the Institute continued its work along the lines already laid down. It soon proved possible to add a 'Photographic Studio' run by Maurice Cookson ('Cookie') (Plate 10), who had already worked with Wheeler for some years. Numerous students attended courses given by various Honorary Lecturers and Professors and special ones by invited lecturers. As many as 84 students are said to have attended the technical courses in 1938/9, but only one research student and two students for the Diploma in Palestinian Archaeology were registered at the Institute in that year.²⁸ Other Diploma students, such as those in Western European Archaeology, were, of course, registered elsewhere. Financially, the Institute's position continued to be fragile, despite efforts to raise more donations and to increase income by undertaking



Plate 10 'Cookie' (Maurice Cookson) developing prints at the village pump at Huelgoat, Brittany (the only running water available), 1938 or 39.

contract work in conservation and photography and by instituting a membership scheme. An application for a grant from central University funds failed. Each year there was a small deficit on running costs which had to be met out of capital and which was obviously worrying for the long term.

Such problems were postponed, however, by the outbreak of hostilities in the autumn of 1939. The teaching activities of the Institute were suspended and the library and collections packed away in the basement. The building suffered some slight damage from enemy action in 1941. Wheeler threw himself immediately into the task of raising an Anti-Aircraft battery and, eventually, in 1941, was posted to North Africa. A year after this the Senate agreed that the Secretary, Kathleen Kenyon, should be appointed Acting Director during his absence.²⁹ She had joined the Red Cross at the beginning of the War but was able to sleep at the Institute and devote some time to its affairs (Plate 11).

Wheeler returned briefly to Britain for a few months in late 1943 and early 1944 preliminary to taking up his new post of Director General of Archaeology in India.



Plate 11 Kathleen Kenyon with Lord Harewood (Chairman of the Trustees of the London Museum) at the opening of an exhibition, 'The Present RedisCOVERS the Past', mounted by the Institute at the Museum in March, 1943.

He was thus on hand for discussions which led to fundamental changes in the Institute's financial position and status in the University during the post-war period. In the autumn of 1943 the Chairman of the UGC asked universities to send in a statement of their needs by the following March. This request was duly forwarded by Senate House to Schools and Institutes, including the Institute of Archaeology. The Management Committee met in December and set up a sub-committee to draft a statement. This consisted of Wheeler, Stephen Glanville (Petrie's successor as Professor of Egyptology), Sir Cyril Fox, Kathleen Kenyon and Miss Whinney of the Courtauld Institute.³⁰ The statement that emerged set out a view of the Institute's future role which differed fundamentally from what had been accepted in the pre-war years.³¹ The Institute, it proclaimed, should be developed as 'the natural and obvious focus' of Archaeology in the University and therefore needed to be properly equipped and staffed for this purpose. While it was not proposed to relocate existing departments, such as Egyptology and Classical Archaeology, 'at any rate at present . . . , although eventually such a step may be found desirable', it was urged that 'new departments of the subject should not be dispersed throughout the University, but should be located at the Institute itself. In this respect', it continued, 'the Institute of Archaeology differs from the Institute of Historical Research and approximates to the Courtauld Institute of Art'. Its teaching was in a considerable measure based on the use of type-collections and there was a common need in all branches for technical training and some knowledge of environmental factors.

Pursuant to this policy which, it was stressed, incorporated the recommendations of the Board of Studies, a number of paid teaching posts needed to be created. First, a full-time chair of Prehistoric Archaeology, 'to incorporate and enlarge the work carried out for fifteen years or more through a part-time Lectureship'. This would be combined with the Directorship of the Institute. Next, part-time Chairs in the Archaeology of Western Asia, India and Environmental Archaeology were specified (to be converted into full-time posts as soon as possible), together with a Lectureship in Palestinian Archaeology. In addition, a photographer and two assistants in the Repair Laboratory would be needed, a full-time Secretary and Accounts Officer, an Assistant Secretary and Librarian, a shorthand typist and a caretaker.

It took more than a year before the results of this exercise were known. The Board of Studies supported the Institute's bid, however, and in the course of 1945 it became known that the Court Grant for Central Activities for the first time included money for the Institute. In the event the grant was not sufficient to implement all the recommendations, so some appointments, including the Chairs of Indian and Western Asiatic Archaeology had to be postponed.

Because of his appointment as head of the Indian Archaeology Service Wheeler was not available for consideration as Director; he had tendered his resignation as Honorary Director at the Management Committee in February 1944.³² A sub-committee was set up in October of that year to advise on the appointment of a Director and at the December meeting it unanimously recom-

mended Professor Gordon Childe, the Abercromby Professor at Edinburgh³³ (Plate 9). Childe was willing to accept, but the confirmation of University funding came too late to allow him to take up his duties until the third Term of the 1945–46 session. Zeuner was recommended by the Management Committee for the part-time Chair in Environmental Archaeology at its meeting in October 1945. He was, of course, already at the Institute.

Other posts followed during the next few years, despite some delays due to fluctuations in funding and other causes. Max Mallowan was appointed to the part-time Chair in Western Asiatic Archaeology early in 1947 and, during the following session, Kathleen Kenyon accepted the offer of the Lectureship in Palestinian Archaeology and resigned as Secretary. The part-time Chair of Indian Archaeology was eventually transformed into a full-time post held jointly with the School of Oriental and African Studies. In the meantime two other academic posts were under discussion between the Management Committee and the Board of Studies. These were in the Archaeology of the Roman Provinces and that of the Anglo-Saxon



Plate 12 The Institute's staff in 1955. *Back row:* Mr and Mrs Dance; Mary Pinsent; Jennifer Banham; Penny Brooks; Joan Sheldon; Judy Philips; Marjorie Maitland Howard; Harry Stewart. *Middle Row:* Marjorie Conlon; Rachel Maxwell-Hyslop; Arthur ApSimon; Ian Cornwall; Gerry Talbot; Olive Starkey. *Front Row:* Maurice Cookson; Kathleen Kenyon; Sheppard Frere; Max Mallowan; Gordon Childe; Frederick Zeuner; Edward Pyddoke; Joan du Plat Taylor; Ione Gedye.



Plate II Gordon Childe lecturing in Central Asiatic dress, 1956. (From a sketch by Marjorie Maitland Howard.)

period. In the end it was decided that these should be divided between the Institute and University College. Because of its considerable teaching collections relating to the Roman period the Institute, though expressing interest in the later period, felt obliged to give priority to the Roman post. This eventually emerged as a further part-time Professorship, which was offered to Wheeler on his return to England in 1948.

Joan du Plat Taylor was appointed to the position of Assistant Secretary and Librarian, initially on a one year contact from November 1945. Not surprisingly, it was very soon found necessary to separate the two halves of this post and Joan Taylor of course remained for many years, until her retirement, as the full-time Librarian of the Institute. A Library Clerk soon had to be added (Geraldine Talbot, who eventually succeeded Joan as Librarian). Maurice Cookson was reappointed as Photographic Assistant and soon after was given an assistant, Mrs M. Conlon. Kathleen Kenyon was succeeded by Ian Cornwall as Secretary, who was in turn succeeded by Edward Pyddoke when he became a Lecturer in Zeuner's Department. By 1955 the staff had grown still further (Plate 12).

The Institute had now assumed the form on which all subsequent developments have been based. Teaching in Prehistoric and Romano-British Archaeology was provided for undergraduates in Anthropology and Ancient History; training in the field and technical aspects of archaeology was available for students registered for Postgraduate Diplomas in Archaeology (e.g. in Classical Archaeology) at University College, SOAS, etc. But students were also registered at the Institute for a number of different Diplomas, including Prehistoric European Archaeology; Western European (i.e. Iron Age and Roman) Archaeology; Palestinian and Mesopotamian Archaeology. These developments did not pass entirely unchallenged. In 1949 a Visitation of the Institute took place in accordance with the University Statutes. The visitors were Professor Bernard Ashmole (the Yates Professor), Professor H. I. Bell and Professor F. Norman. While appreciative of the Institute's facilities and its work, they nevertheless felt that the post-war policy of expanding the Institute's teaching should be reconsidered. 'The alternatives open to it are to continue as a teaching body on the model of the School of Slavonic and East European Studies, or to become a research Institute on the model of the Institute of Historical Research . . . In our opinion the undergraduate teaching function, except in respects [sic] of fieldwork, photography, drawing and other practical matters, should be allocated to Schools of the University, and the Institute should concentrate on postgraduate research work, the maintenance of its invaluable collections, the photographic and technical departments and a central administrative organisation, and its main function should be to provide facilities for the advanced work of all teachers of archaeology within the University. Special courses of lectures should be given, but the teachers should be drawn not only from the Institute, but also from the Colleges, *to which some Members of the present teaching staff will have to be attached*'³⁴ (author's italics). In other words, a return to the 1930s! This recommendation aroused strong reaction in the Institute's Academic Board and Management Committee, which

stressed the undesirability of separating the undergraduate teaching from the relevant collections and pointed out the contradiction inherent in expecting the Institute's work to continue at a high level, while removing the academic staff.³⁵ The move was successfully resisted and, with hindsight, I think we can be thankful. Had it succeeded, later expansion might have been less rational and subsequent contraction less so still.

It had always been intended that the Institute should ultimately be accommodated on the Bloomsbury site. In 1949, however, it became evident that the Crown Lands Commissioners, under pressure from the Ministry of Works, wished to terminate the Institute's occupation of St John's Lodge as soon as possible after the expiry of the original fourteen-year lease in 1951. The Ministry, it turned out, had ideas of using the building for Government entertainment and for 'a rather better class restaurant', as they put it, for patrons of the open-air theatre.³⁶ Protracted negotiations ensued on various fronts. Eventually, early in 1953, the University



Plate 13 No. 31 Gordon Square. (From a watercolour).

offered some of the houses on the north side of Gordon Square (Plate 13). At first the idea was to adapt the existing buildings, with the addition of 'two Nissen huts . . . in the backyards' for the Technical Department and a students' Common Room.³⁷ However, a few months later space was also offered to the Institute of Classical Studies on this side of the Square, at which point the Court 'felt that it must choose a single architect who could plan the whole of the North side'.³⁸ The result was our present building, shown in Plates 14–16 at various stages of its construction.

Gordon Childe retired a year early, at the end of the 1955–56 session, in order to give his successor the opportunity to supervise the later stages of the construction and arrangement of the new premises. It was a real sacrifice, for Childe, the greatest archaeological theorist of his time, lived entirely for his work and, with his post, he gave up, quite literally, his reason for living. A gentle and kindly, though somewhat eccentric man, he inspired great affection as well as admiration and he had a puckish sense of humour. It was typical of him that he chose to give one of his farewell lecture series dressed in a gaudy Central Asian costume acquired during a recent visit to the USSR (Plate II). He had, however, no real interest in administration and his awareness of this was no doubt a factor in his decision.

It was decided that the Directorship should be separated from the Chair of Prehistoric European Archaeology. W. F. Grimes ('Peter' Grimes, as he was universally known), who was appointed Director (Plate 17), was an experienced and conscientious administrator, and a field archaeologist of note. As Wheeler's successor at the London Museum he had been on the Institute's Management Committee since 1943 and had played a considerable part in planning the new Institute building. Under his guidance the preparations for the move were made and the Institute eventually took possession of its new premises early in 1958. The building was formally opened by the Queen Mother, as Chancellor of the University, on 29 April, the same day as that on which the Ceremony at St John's Lodge had taken place in 1937 (Plate 18).

The club-like atmosphere and delightful parkland setting of St John's Lodge were much regretted by many, especially the older members of the Institute staff. The rather more impersonal character of the new building did not please them and Kathleen Kenyon almost resigned over the ban on bringing dogs into it, but the move improved the Institute's position in a number of ways. It was no longer the rather isolated community it had been in Regent's Park but a part of the central nucleus of the University, close to several of its institutions with which we had close links and also to the British Museum. It had generous new purpose-built and specially planned accommodation, which provided space for future expansion. This has served us well, though we have long ago used up the extra space.

Since we came here rather more than a quarter of a century ago there have been many changes, apart from those in personnel, which I can mention only in summary fashion. We have progressed essentially along the lines laid down in the early 1940s as the central focus of archaeology in the University. This has involved



Plates 14 and 15 Plate 14 shows the stage reached in the construction of the new Institute building on 28 May, 1956; Plate 15 the position on 13 July, 1956.



Plate 16 The Institute's new building nearing completion. Photo taken on 6 November, 1957.

not only the gradual building up of staff, but also two other developments which were not foreseen at that time, namely the introduction of the BA and BSc degrees in Archaeology in the late 1960s (which involved the registration of undergraduates at the Institute for the first time), and the great expansion in the science element in our work, particularly during the last two decades.

Of these three trends, the first involved working towards a better balance of academic staff, as well as the filling of important gaps. For the first few post-war decades the Institute had, like the Texan army, too many generals. As student numbers grew and the subject itself developed, the disadvantages of this became increasingly obvious, but could only be very gradually rectified. At the same time, posts relating to new areas or disciplines were also regularly planned for in Quinquennial proposals, while the Quinquennial system lasted, but were sometimes only achieved by indirect means. For instance, it was the Parry Report on Latin American studies which allowed the creation, in 1967, of a long-desired lectureship in New World archaeology – now a Readership and still the only post of the kind in this country. More recently we have been successful in acquiring three new posts under



Plate 17 W. F. ('Peter') Grimes.

the national 'New Blood' and the University's New Academic Initiatives schemes. On the other hand, the financial difficulties of recent years have inevitably resulted in some losses, most notably of the only Chair in Western Asiatic Archaeology in Britain.

The introduction of first degrees in archaeology in 1968 was a joint initiative of the Institute and the Board of Studies in Archaeology. It had been a number of years in gestation, chiefly because of a strong prejudice, both in the University at large and among some teachers at the Institute, against archaeology as an undergraduate subject. By the late 1960s, however, the development of undergraduate courses at a number of other universities in the country had made our Postgraduate Diplomas something of an anachronism and we have certainly had no reason to regret taking this step, which soon brought a flood of good applicants, and continues to do so. The initiation by the University of taught Masters degrees at about the same time



Plate 18 The Queen Mother, Chancellor of the University, talking to Marjorie Maitland Howard during her tour of the Gordon Square building after the opening ceremony on 29 April 1958.

provided the opportunity to introduce courses which have proved to be a popular alternative to the old Diplomas, especially with overseas students.

Wheeler's concern with training in fieldwork and recording skills has not been forgotten in all this. An Institute field-training course was introduced by Grimes in 1958 and since 1974 this has been run by our Field Unit, which also offers many other opportunities to students of acquiring further field experience.³⁹

In many ways the most remarkable development during this period, and particularly during the last two decades, has been the growth of the Institute's involvement with what has come to be known as Archaeological Science. In this we have been in the forefront of a more general trend in the subject, aided by our position as the acknowledged centre of archaeology in the University and by our early established tradition of including scientists on the staff. We were thus well placed to benefit from the input of funding into this increasingly important area of

archaeological work as a result of the setting up of the SERC's Science-based Archaeology Committee a little over a decade ago.

After just over half a century as a Senate Institute, we have just begun a new phase of our existence as a part of the University College London. Though mergers (a plague on the word!) have become part of our daily lives in the University for some years past, this one is more a unification of long-separated parts of the same field; a reintegration of Archaeology as a whole with related disciplines in both the Arts and Science Faculties of the College. The process was certainly triggered by financial pressures, but the academic advantages to both sides were soon seen as the decisive factor. The symbol of this is the new Archaeology and Ancient World Studies Committee which has been created in the College to ensure that they are fully exploited. At the same time we continue to function as the University's Institute in the field of archaeology for the benefit of the University as a whole. On this new basis I think we can face the prospect of the next half century with quiet confidence.

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4. Senate Minutes, 22 November 1905, 322–25; 24 January 1906, 859. See also C. H. Read in *Man* 38, 1906, 58f. (I am indebted to Professor J. G. D. Clark for first drawing my attention to Read's article on these degrees).
5. Wheeler, *ibid.*, 66.
6. *Ibid.*, 72.
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21. IAMC Minutes, 28 April 1937 (Director's Report). Among other benefactors were Sir Robert Mond, Mr Ernest Makower, Sir Percival David and Mr A. L. Rickett.
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24. *Ibid.*
25. IAMC Minutes, 2 July 1935.

26. Wheeler, *ibid*, 112.
27. IAMC Minutes, 24 November 1936.
28. Second Annual Report of the University of London, Institute of Archaeology, p. 9; 'University of London Institute of Archaeology: Historical Note and Statistics prepared for the Information of the University Grants Committee' n.d. [1949].
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31. 'Statement of needs for submission to the University' (Bound with IAMC Minutes, vol 1, 1934–56).
32. IAMC Minutes, 3 February 1944.
33. *Ibid.*, 11 December 1944.
34. University of London. Visitation under Statute 114: Institute of Archaeology. Inspector's Report, 1949.
35. Institute Academic Board's Comments on Visitors' Report, 1949.
36. IAMC Minutes, 2 October 1951.
37. *Ibid.*, 5 May 1953.
38. *Ibid.*, 9 June 1953.
39. This side of the Institute's activities was fully dealt with by Dr Peter Drewett, Director of the Institute's Field Unit, in his lecture in this series on 27 January 1987.



Plate 19 HRH The Princess Anne, Chancellor of the University of London, on the occasion of her visit on 29 April 1987 to unveil a plaque in commemoration of the Institute's Jubilee. With her are the Director, Professor John Evans (left) and Sir James Lighthill, Provost of University College.

ACKNOWLEDGEMENTS

I am grateful to the University of London Library for permission to reproduce the plan for an Institute of Archaeology (Plate 3), and to the staff of the Library's Palaeography Room for their help in consulting the relevant portions of the Archives of the Central Offices.

Memories: The Institute, 1946–1947

by GERRY TALBOT

I first went to the 'Old Institute', at St John's Lodge in Regent's Park in the spring of 1946 on a voluntary basis. I helped Joan du Plat Taylor who had taken on the post of Librarian from Miss Patchett, the pre-war Librarian. Shortly after the arrival of Professor Gordon Childe in October of that year I was taken on as regular staff.

St John's Lodge was a large Regency villa which had belonged to the Marquess of Bute. It had splendid spacious rooms, particularly the ballroom (which was used for public lectures) and the library, which had large french windows opening on to the garden. Seminars and lectures were given here during the evenings to suit the part-time students, many of whom had daytime jobs. Joan and I worked in a room which housed the periodicals and led off the main library. Later we moved to the little circular room which had once been a chapel. However, the room I remember with most pleasure was the sort of anteroom to the ladies' loo. It was circular and entirely panelled with looking glass and had elegant benches round the walls, but the chief attraction was the grating in the floor through which rose gorgeous warm air. This made it the most popular room in the building for the female staff and students, especially during the extremely cold winter of 1946/7, when we skated on Regent's Park lake during the lunch hour.

Though the house was rather inadequately heated, the general atmosphere was delightful, friendly and completely informal; staff and students all mixed together, and visitors too, so that it really was a club for archaeologists. There was a large garden and, though open to the public, it was very rarely visited by outsiders, probably because it appeared to be the private garden of St John's Lodge. During the summer some of us – especially Ian Cornwall – had sandwich lunches in the garden and we saw a great variety of birds, even sparrowhawks. The gardeners were very friendly and gave us surplus plants. I once bicycled to my home in Chelsea with a bundle of pea-sticks slung across the handle bars, rather to the frustration of the rest of the traffic.

We were not far from the Zoo and could hear the animals quite plainly. One day we noticed some mysterious footprints going vertically up a pipe and on to a gallery round the entrance hall. Clearly not a cat, so the Zoo was rung up and yes, a rather vicious monkey had escaped and the keeper would come at once; we must not try to catch it as it would undoubtedly bite. So two men arrived with nets and after a chase managed to catch it. Luckily Kathleen Kenyon's dogs were not there that day.

In the autumn of 1945 some of the pre-war staff returned to the Institute after concluding their war work. Kathleen Kenyon had spent the war at St John's Lodge while carrying out

her Red Cross work in London. She had been Secretary since the foundation of the Institute and was appointed Acting Director when Rik Wheeler became Director General of the Archaeological Survey of India. He subsequently returned to the Institute in 1948 as Professor of the Archaeology of the Roman Provinces. Margot Eates resumed her post of Assistant Secretary and Maurice Cookson returned to the Photographic Department when demobilised from the RAF. Frederic Zeuner continued to run the Department of Geochronology and Ione Gedye resumed her conservation work in the Repair Department, as it was then called, where she was joined by Olive Starkey. Rachel Maxwell Hyslop took up her pre-war appointment of Assistant in the Department of Western Asiatic Archaeology, as did Kitty Richardson in the Department of Prehistoric European Archaeology. And last, but not least, was our splendid caretaker Manson and his wife and daughter; he had spent the war at St John's Lodge and, with Kathleen Kenyon, had put out many fire bombs dropped in Regent's Park.

In September 1946 Gordon Childe arrived to take up his post as Director and Professor of Prehistoric European Archaeology. Max Mallowan joined the staff in April 1947 as Professor of Western Asiatic Archaeology. In addition to the staff, Olga Tufnell had a room where, with David Diringer she worked for many years on the Lachish reports which were finally published in four volumes. Oliver Myers also had a room for a time when he was working on the Wellcome Marston material. Margaret Murray was a frequent visitor to the Library.

In the early days after the War there were very few full-time students; in fact they were almost outnumbered by the staff. At that time no first degree was conferred, the Post-Graduate Diploma being the main objective. Records of the period are practically non-existent – the only register surviving is a book in which the names of the students at Professor Childe's classes are listed in his own handwriting, though the first three of these have no date. The first consisted of six students: Mike Maitland Muller, Edward Pyddoke, Nancy Sandars, Grace Simpson, – Salmond and Hilary Walton. Nancy Sandars tells me this was 1946/7. The next list, also undated but evidently 1947/8, consisted of Sinclair Hood, Leslie Willis, Meg Whitley, Leslie Grinsell, Humphrey Case, – Gwatkin, Miss Einars (from Iceland) and Mr Maraspini (from Egypt). These last two were the first diploma students from abroad; there were also many foreigners in the Technical Department, but no register exists. The following year Jay Butler, who had come over from the USA to study dentistry, was sent by mistake to a lecture by Childe at the Institute; though somewhat startled he was immediately hooked on archaeology and became a very efficient and well-known archaeologist.

In addition to the full-time students some, such as Judy Philips, were part-time and came to the evening lectures. Also some undergraduates from University College attended lectures; these included James Mellaart, Diana Kirkbride (now Helbaek) and Vassos Karageorghis. Some of the lectures were quite exciting: I remember Louis Leakey, in order to illustrate prehistoric hunting, produced a stuffed hare which he proceeded to shoot at with a primitive bow and arrow. Demonstrations of flint making were generally a rather gory affair. Some years later Professor Childe appeared to give a public lecture draped in a white sheet, doing a penance for some statement he had made. Never a dull moment at the Institute in those days.

Bloomsbury, Baghdad and Beyond

by PETER J. PARR

When I first began, reluctantly, to contemplate this lecture, I decided to do what all archaeologists are prone to do – divide it into three parts. Three, I hoped, would prove a lucky number; it was also the total number of suitable words beginning with ‘B’ I could think of for the title. However, we all live and learn, especially if we attend lectures; sitting in this room on Tuesday evenings over the past few weeks I have learned a great deal about archaeology and, more interestingly, about archaeologists. Each Tuesday what I heard prompted me to rush home and write yet another draft of my own contribution to this series, in order to incorporate some new thoughts and, hopefully, some new wisdom, so much so that the paper was in danger of losing its tri-partite-ness and becoming just a muddle. Probably that is what you will judge it to be still; though I have at least tried, in this final version, to reimpose some sort of order, but only by dispensing – except in respect of one matter – with all the benefit of the new insights I had obtained from my predecessors at this rostrum. That one matter mentioned is of such particular importance to my theme that I shall begin with a sort of philosophical prologue devoted to it, before turning to the more prosaic and practical aspects of Near Eastern archaeology which is my central concern.

The exception relates to last week’s occasion [17 March 1987]. I am sure that all of us received much food for thought from the papers given by Ian Glover and Warwick Bray; I know that the corridors and seminar rooms of the Institute are already buzzing with the discussions to which their remarks have given rise. They were asking the question, you will remember, whether British archaeologists working abroad, and especially in the Third World, were carrying out what could honestly be termed ‘objective scholarship’, or were indulging in a kind of cultural imperialism and exploitation, appropriating other peoples’ pasts for their own rather nefarious purposes. Should we not leave ‘foreign’ archaeology to ‘foreigners’; were our academic journeys abroad really necessary? These are important questions, which will rightly provoke much heart searching.

The papers of Drs Bray and Glover troubled me most of all, not because of the doubts expressed therein concerning British involvement in archaeology abroad, but because I had never experienced such doubts myself, despite the fact that I had spent the past thirty-five years of my life practising archaeology in foreign parts. I went home last Tuesday worried because I had never had these worries. Was this, I wondered, because I was particularly insensitive, dull and stupid? Was it because I did not belong to that fortunate group of active British archaeologists abroad, referred to by Dr Bray, who were still under the age of fifty?

I could still remember the British empire; perhaps I was irredeemably corrupted by the memory. Well, no doubt all this is true; but on more sober reflection I concluded that these were not the real reasons for my hitherto lack of concern. That, I decided, was more to do with the fact that never, whilst studying the archaeology of the Near East, had I thought of myself as studying someone else's past; I was studying my own. Nor had I ever been made aware by my hosts and colleagues in that part of the world that I was an intruder, poking my trowel into 'their' exclusive heritage. We had not always seen eye-to-eye, but there had always seemed to be a mutual understanding that what we were all doing was seeking to uncover and understand a heritage that was common to us all. Attitudes in the Middle Eastern countries towards archaeology are certainly changing and there are countries where we are possibly not as welcome as we once were. However, for the present, I feel sure that most scholars there, of whatever nationality, accept the fact that, whether they or we like it or not, their countries were the setting for most of those really major developments in the material, cultural and spiritual history of mankind which still affect the behaviour and thinking of not only them but of us. I refer, of course, to the agricultural and urban revolutions; to the birth and consolidation of those canons of artistic and literary expression on which all later Western art and literature depends and which, fortunately or unfortunately (depending on one's point of view), now dominate a large part of the non-Western world also; to the invention of writing and the principle of the alphabet; and to the birth of the three great world religions which have spread most widely around the globe. This is by no means to deny that there were and are other great centres of civilisation elsewhere in the world which have heritages of their own, which are of the utmost significance to those peoples who feel they belong to those heritages, and of legitimate interest to others who do not. But surely, when we look at the world as it is today, dominated by Western technology and lifestyles, we cannot deny the special importance of the ancient Near Eastern tradition. Childe's *ex oriente lux* may be deplored as ethnocentricity, though, if so, it is ethnocentricity very broadly defined; but I believe it to be our first duty as archaeologists and historians to understand ourselves and our own beginnings, and in this I believe also that we can continue for a long time to count on the co-operation and encouragement of our colleagues in the Middle East to further our common ends. If Drs Bray and Glover have any doubts about the validity of their own involvement in Third World archaeology, let me invite them to transfer their efforts to the Near East, where their motives will be seen to be as pure as the desert sands.

Having demonstrated – at least to my own satisfaction – the importance of Near Eastern archaeology (a term, incidentally, which I much prefer to Western Asiatic and which I shall henceforth use) let me now turn to the main part of my lecture, which will be in the nature of a series of personal, perhaps not fully thought out, reflections on some aspects of the discipline today, stressing its problems and hopefully suggesting some ways in which they might be solved. As you have gathered, the lecture may be divided into three parts. In the first section I shall review briefly the history of this Institute's Department of Western Asiatic Archaeology, though not exhaustively, since I believe that a Jubilee should not primarily be the occasion for nostalgic and sentimental eulogising. It should rather be an opportunity to reflect upon the lessons to be learnt from past fortunes, and even more from past misfortunes, in order to be

better equipped to face the future. If I have more to say about the shortcomings and omissions of the past half century than about the achievements, therefore, I shall beg no forgiveness. This part of my talk you may understand as forming the 'Bloomsbury' of my title, and if you protest that for the first 21 years of its life the Institute was not in Bloomsbury but in Regent's Park, I shall answer that it was always *intended* to be here, as Professor Evans made clear in the first of these Jubilee lectures.

I shall then hope to take you, in spirit, to Baghdad, from whence we can contemplate in a broader way some of the opportunities and problems existing in Near Eastern archaeology today and consider the part which British scholarship generally, and this University specifically, can play in exploiting and solving them. Baghdad is perhaps not the most tranquil or comfortable of cities at present in which to mediate on these matters, and so we may be thankful that we can do so *in absentia*, from the relative comfort of this lecture hall. Nevertheless, Baghdad lies at the heart of that part of the world which is our interest and, as such, it provides an obvious focus for our thoughts. It is also the seat of one of the most illustrious of our British Schools of Archaeology abroad, a fact which will make it difficult for me to avoid, in this second part of my lecture, some comments on those institutions and thus on the whole question of public funding of Near Eastern archaeology.

As for Beyond – this is a vague concept, which can be interpreted in a number of different ways – geographical, chronological, even metaphysical. What I shall have to say about the Beyond I am not yet sure, but I comfort myself with the thought that it is still some time before I need come to this, the third part of my talk, and I am hopeful that something will occur to me before then.

Although the first specific reference to a Department of Western Asiatic Archaeology at the Institute did not occur – so far as I can discover – until after the War, the close connection of the Institute with the Near East goes back right to the very beginning, as is now well-known. Professor Evans told us a few weeks ago of the decisive role played by Sir Flinders Petrie's collection of Palestinian antiquities and the financial donation from Mrs Wharrie which accompanied it and which ensured the Institute its first home at St John's Lodge. I shall have no more to say of this, though I shall return to the matter of the collections presently. The Near Eastern connection went far beyond this, however. The Institute was founded deliberately to cater for new needs and to encourage the development of new fields of research. High on the list of these were (if we are to believe the Chancellor's words at the Inaugural ceremony) those of Mesopotamia, Syria, Palestine and Cyprus, the archaeology of all of which was said (rightly) to be much neglected in British universities at the time. There was even reference to an 'appeal recently made by the Archbishop of Canterbury and others for the endowment of a teaching post in Palestinian or Biblical Archaeology, with [the Petrie] collection as its basis', an appeal which had, apparently, been launched with 'a generous initial donation' from Sir Charles Marston. (We have some objects in the collections given by Marston, but where the money went to I have no idea.) Nor were these mere words; already by the time of the Second Annual Report for the Session 1937–8 two of the nine members of the academic and administrative staff listed were Near Eastern specialists, Professors S. H. Hooke and Sidney Smith. Neither of these distinguished scholars was, of course, 'staff' as we know them; they were

outside teachers 'brought in' to cope with a specific demand, Professor Hooke from King's College (where he was Davidson Professor of Old Testament Studies) and Sidney Smith from the British Museum. The demand they met was that created by the Institute's first two students, who were, as Professor Evans also reminded us, Barbara Parker and Rachel Clay, later Mrs Maxwell-Hyslop. Again, in this Second Annual Report, it is stressed in no uncertain terms that 'the principal need at the present time is in Near Eastern Archaeology, and the establishment of the teaching of this subject is amongst the first aims of the Institute'. One suspects indeed that these are the words, not of the Director himself, since Wheeler's own interest in the Near East at this time could hardly have been very great, but of the Secretary, Kathleen Kenyon, who had already served with distinction her archaeological apprenticeship at Samaria in Palestine, and was now hard at work classifying the Petrie bequest.

Good intentions were frustrated by the War, however, and it was not until 1946 that an appointment was made in what was for the first time – as I have already mentioned – termed the Department of Western Asiatic Archaeology. The appointee was Rachel Maxwell-Hyslop, as a 'Part-time Assistant' a role which, according to her, involved mostly cataloguing the already growing collections. Most significantly, however, was the fact that in the University's grant for that year financial provision was made for a Chair in Western Asiatic Archaeology; in April 1947 Max Mallowan was appointed to this, thus taking his place on the Professorial staff with Gordon Childe and Frederick Zeuner, both of whom had been appointed the previous year, and with Professor Codrington, appointed in 1947 also. A few months later, in January 1948, Kathleen Kenyon received the Lecturership in Palestinian archaeology and the Western Asiatic Department can truly be said to have been born.

I promised I would not inflict upon you a laborious and detailed history of the Department, its members and its activities, and I intend to keep that promise, but I would like to linger for a moment in order to comment on the achievements of these two giants, Mallowan and Kenyon. They undoubtedly set the course on which the Department has proceeded ever since and endowed it with its distinctive ethos. Both were, first and last, field archaeologists: Mallowan had, since 1932, been directing excavations in Iraq and Syria at sites such as Chagar Bazar, Tell Brak and Arpachiyah, and although Kathleen Kenyon had hardly begun her career in the Near East, she had proved herself in the field to Wheeler's satisfaction at a number of Iron Age and Roman sites in Britain. They thus conformed entirely to the spirit of the Institute as conceived by Wheeler, as Peter Drewett pointed out in his lecture a few weeks ago. This is not to deny their contribution to what many non-field archaeologists are prone – wrongly, of course – to think of as more academic or scholarly aspects of the subject. Both Mallowan and Kenyon went on to make important synoptic contributions to our understanding of the prehistory and early history of the Near East, but their interpretations of the past, however intuitive and imaginative we may sometimes consider them to be, were firmly based on what they saw to be the facts. These were primarily the facts recovered in the field and they would, I believe, have considered time spent on theorising as so much time wasted. They took their discipline to be a humanistic one, though this does not mean that they were unmindful of the contributions the natural sciences could make to the subject. They would never have recognised it as a 'social science', however, and would not, I fancy, have liked the 'New

'Archaeology' had it then been around. In these attitudes I happen to think that they were largely right, but I do not want to discuss or defend them now. My point is that they established a pattern which members of the Department have largely followed in their work ever since. It is not my intention, nor do I have the time, to discuss that work here; it figures largely in the many histories of Near Eastern archaeology which have been written, not least in the autobiographies of Mallowan himself and of his successor to the Chair, Seton Lloyd. I wish instead to draw attention to what is a more unhappy aspect of the Department's later history.

For the first thirteen years of the Western Asiatic Department's life it comprised its founder members – Max Mallowan, Kathleen Kenyon and Rachel Maxwell-Hyslop, who had been promoted to Lecturer in Mesopotamian Archaeology and was well established in her special field of ancient Near Eastern metallurgy. In 1960 she resigned, to be replaced by Barbara Parker, recently returned from Iraq where she had taken part in many of Mallowan's field campaigns. At the Institute she was to contribute her special interests in the study of seals and of Mesopotamian religion to the teaching activities of the Department, while Mrs Maxwell-Hyslop also continued on a regular basis to conduct metallurgical seminars. It was not until 1962 that big changes took place, with the resignation of both Mallowan and Kenyon and the appointment of Seton Lloyd and myself. In that year, therefore, the academic staff of the Department numbered four – the professor, two lecturers and a 'special tutor'. But this was still a time of expansion: two years later James Mellaart took up his appointment as Lecturer in Anatolian Archaeology, bringing the total to five, a figure not disturbed by the replacement in 1969 of Seton Lloyd by another distinguished field archaeologist, David Oates. The range of specialisms now being taught within the Department and of the research activities of its members had never been wider, far oustripping those of any other university department in Britain with an interest in the ancient Near East. The intention of the founding fathers twenty-five years before – to encourage and develop new fields of research and teaching – seemed to have been largely achieved so far as the Near East was concerned.

Halcyon days, however, cannot last forever and the clouds gathered. For the Western Asiatic Department these began to appear as early as 1976 with the retirement of Barbara Parker, to be followed three years later by that of Rachel Maxwell-Hyslop from her tutorial post. Neither were replaced and the Department thus suffered two grievous losses, perhaps inevitable, given the climate of the time, but none the less detrimental to the unique kind of specialist teaching and research that had been developing over the previous decade. These losses were, however, nothing to those which occurred in 1982. With the early retirement of David Oates at the end of that session, not only did the Department lose its surviving Mesopotamian specialist and its administrative Head, but the University also lost its Chair of Western Asiatic Archaeology, the only one in the country. There ensued what can only be described in my opinion as a quite appalling situation, especially as it became clear that the term 'frozen' adopted in connection with the Chair and the Mesopotamian specialist was, given the circumstances of Professor Oates's retirement and under the terms of the forthcoming amalgamation with University College, little more than a euphemism. The posts were lost and their re-establishment, either united, as they had hitherto been, or separated, would, we were led to understand, have to be negotiated again *ab initio*. So far as Mesopotamian

archaeology was concerned – that branch of the discipline which forms the heart of ancient Near Eastern studies and the core of the Department – we were literally back to square one. The fact that since 1982 the Department has been able to some extent to meet its commitments to students has been entirely due to the valiant and under-rewarded efforts of a veritable phalanx of friends and colleagues from outside the University – Harriet Crawford, Georgina Herrmann, Julian Reade, John Curtis and Dominique Collon – who deserve a public vote of thanks. It is indeed ironic that, in our reliance on such outside teaching to continue functioning in this field of study, we have been obliged, as a survival strategy, to do what our founding fathers did half a century ago when they traded upon the good nature of Professors Hooke and Smith. Our various attempts so far to rectify this situation have been to no avail, and there seems little sign of any immediate relief. Whether the present unsatisfactory state of affairs can continue for much longer without intolerable strain being suffered by the Department and without permanent damage being inflicted upon Near Eastern archaeology in this University is, to my view, highly doubtful.

At this point, remembering some of Dr Bray's comments last week about making amends for a previous over-concentration on 'ethnocentric' archaeology and the need to redress the balance, I must, of course, pause to ask whether the losses in the Western Asiatic Department were all that important, in the wider view, and ignoring the purely Departmental inconveniences. In my earlier defence of Near Eastern archaeology I have already largely answered this question, but I must add here that it is not only the discipline in Bloomsbury that has now suffered, but throughout Britain as a whole. In the universities of the United Kingdom at present there are only five permanent teaching posts tied by title to Near Eastern archaeology (only two of which, incidentally, are held by specialists in Mesopotamian archaeology of the historical periods). Five is not, I believe, an excessive number to cope with a temporal span of some nine millennia, from the Neolithic to Alexander the Great, over an area stretching from the Black Sea to the Indian Ocean, and the Mediterranean to the Iranian plateau; an area from which, over the past century or more, an enormous quantity of archaeological material has been recovered. There may be room for rationalisation in Near Eastern archaeology in the universities, but there is no case for reduction.

Let me now turn to a more cheerful matter and look briefly at the teaching activities of the Department. Since I have not collected the relevant statistics I shall not discuss student numbers except to say that, rather surprisingly and gratifyingly, there does seem to have been a steadily increasing interest over the years in Near Eastern archaeology, both among those students taking taught courses and those doing research. But as regards *what* we have been teaching, I think something more needs to be said, since this is an area where we can certainly learn from the past and apply it to the future. Until 1968, when the first degrees in Archaeology were introduced, the Department offered courses leading to the three separate Postgraduate Diplomas in Mesopotamian, Palestinian and Anatolian Archaeology, each reflecting, of course, the individual interests of its staff. I have no proof of this, but I suspect that the initial decision to teach at such a specialised level, rather than to offer courses leading to a more generalised qualification in Near Eastern Archaeology, had less to do with academic or educational philosophy than with the status and – may I say? – the personal rivalry of our first

two eminent members of staff, Max Mallowan and Kathleen Kenyon. Be that as it may, these were postgraduate courses, necessarily of an advanced nature, and they gave the Department its unique reputation throughout Britain as a centre for specialised Near Eastern archaeological instruction and research. With hindsight, however, I am convinced it was a mistake to translate these postgraduate diploma courses directly into undergraduate degree courses when the latter were instituted in 1968. The suitability of archaeology as a first degree subject was at the time hotly argued, as many of you will remember, but the battle has been won and few of us would now disagree with the concept of undergraduate archaeology as advocated, for example, in the recent submission to the UGC by SCUPHA (the Standing Committee of University Professors and Heads of Departments of Archaeology). The very fact that archaeology is 'a broad-based and demanding academic discipline [which] encompasses many of the traditional approaches of the Humanities and Social Sciences and requires a wide range of scientific concepts, methods and skills', to quote that document, makes it imperative that a first degree be constructed with care. This, I believe, is all the more so in the case of Near Eastern archaeology, where somehow the student not only has to digest a large body of information about the ancient world but also has to learn how to use critically the very diverse sources of that information. This ranges from radiocarbon determinations and archaeobotanical data, through stratigraphic and typological analysis, to Sumerian texts and the Hebrew scriptures. To subject first degree students to four years – as we did from 1968 to 1983 – of intensive study of the archaeology of an area as small as, for example, ancient Palestine, seems to me, on reflection, to be almost indefensible; especially as, at the same time, we expected the same students to become acquainted with at least some of the exciting developments in environmental and technological archaeology which were, in the sixties and seventies, transforming the whole discipline. During the past decade several schemes have been devised by the Department to modify and improve the suitability of its undergraduate teaching without losing the specialisms for which it had become noted, culminating in 1983 in the introduction of the present modified Course Unit degree, hopefully a more acceptable general education qualification than were its more narrowly conceived predecessors. I do not myself believe we have yet hit on the right solution. I hope that one of the first results of the closer meeting of minds which must ensue from the amalgamation with UCL and the creation of the Archaeology and Ancient World Studies Committee will be a review of the content of our BA degree, so that we can make improvements as soon as our staffing problems have been solved. At the same time, we must not neglect our more specialist teaching, which is of growing interest to overseas students in particular and which perhaps is more appropriate to the MA level.

I have already alluded to the range of specialisms within the Western Asiatic Department as being, until recently, one measure of its unique standing in British universities and I would now draw attention to another. I refer to its teaching and research collections. The basis of these, and still the largest component, is the Petrie material mentioned earlier, comprising the bulk of the pottery and small objects discovered by Petrie between 1926 and 1938 at the three major southern Palestinian *tells* of Ajkul, Fara and Jemmeh, and covering in time approximately the 2000 years before the birth of Christ. Petrie is often praised, rightly, for the promptness with which he published his excavations, but it has to be admitted that his

publications were mostly inadequate, even by the standards of his day. However, the very unsatisfactory nature of his reports – inaccurate drawings of pottery, incomplete analysis of provenances and stratigraphy, etc. – has clearly made our Petrie collection of inestimable value to later scholars, since any proper understanding of the cultural and historical role of these sites entails a re-working of the excavated material. Indeed, our collections have provided the source material for a number of PhD dissertations, emanating both from our own Department and from those of other universities here and abroad. Nor does the Petrie collection stand alone in this respect. Since the Department was founded it has received donations of material from Mallowan's excavations at Arpachiyah, Chagar Bazar, Tell Brak and Nimrud, from Woolley's at Tell Atchana and Al Mina, from Kenyon's at Jericho, and from Seton Lloyd's at Beyce Sultan, as well as many other gifts. Much of this also is still less than adequately published, if at all, and provides a rich mine of information for research students at every level. Of equal importance, of course, is its value as demonstration material for taught degrees; nowhere else in this country, or indeed in many other countries, if I am to believe our overseas students, can BA and MA students handle so readily and so frequently the actual material remains of those ancient Near Eastern peoples which they come to our building to study. Unfortunately, owing to the increasing demands on space in this building, the Collections have not been as accessible in recent years as they were in the past. We must hope that when the new Conservation Centre is established the situation will change, and this vast store of information concerning ancient technology and daily life will begin to play the part it deserves in our activities.

Some of you may have noticed that I have said little so far, except in passing, of the research carried out over the years in the Department. The omission may be thought particularly surprising in view of the fact that recently many of us have been obliged to spend a great deal of our time composing departmental 'research profiles' for the edification of such bodies as the UGC and the University Faculty of Arts. Such profiles are, however, difficult to construct for Humanities departments, where traditionally research has been the largely private activity of individual members of staff and students and has covered a wide and disparate range of topics. When PhD dissertations in the Near Eastern field have dealt with such subjects as ancient Mesopotamian food and drink, Assyrian armour, prehistoric settlement patterns in the Jordan dessert, Bronze Age military architecture in Palestine, and the chronology of Troy, it is hard to claim that any discernible profile immediately meets the eye. Much of the research carried out by members of the staff of the Western Asiatic Department, however, has not been of an individualistic nature, but has involved team work and is comparable in some ways to the team research carried out by, for example, departments of chemistry or physics. I refer, of course, to those field expeditions to the Near East led by Max Mallowan, Kathleen Kenyon and their successors and it is to this type of research activity, to its challenges and problems, that I now wish to turn.

In the context of a lecture celebrating fifty years of work in the field of Near Eastern archaeology by this Institute, perhaps the most interesting thing to note about the enormous amount of scholarly activity which excavations such as those at Nimrud, Jericho and Jerusalem represent is that none of it, strictly speaking, was Institute or University research. To be sure,

the directors of the various projects were at one time or another members of the Institute staff and made use of what facilities were available at, and freely offered by, the Institute; though in the early years of the Department, at least, Max Mallowan and Kathleen Kenyon were employed only on a part-time basis and were expected to carry out their fieldwork in, as it were, their own time. So far as I am aware the expedition of Veronica Seton-Williams in the late fifties to Tell Rifa'at in northern Syria, in which she was associated with Joan Du Plat Taylor and Margaret Munn Rankin, was the only field project in the Near East to which the Institute's name was officially attached, prior, that is, to 1975, when my own excavations at Tell Nebi Mend began. This may perhaps seem to be a matter of little importance – so long as the work was done, who cares who sponsored it? – but I shall argue later that I think it does matter, and that the Institute should play a greater part in field activities overseas. The reason for this hitherto almost total lack of official Institute involvement in Near Eastern fieldwork is, of course, obvious; it is the existence throughout the region of those centres of archaeological excellence, the British Schools and Institutes Abroad. Since it is through these that whatever official policy there is in Britain towards archaeology in the Near East is formulated, and what public funding there is is channelled, and since I wish to say something about both of these matters, it is appropriate that I begin with a few words about these establishments.

Their history is well-known. Of those in my area, the first to be established was the British School in Jerusalem, established immediately after the First World War in 1919, to be followed thirteen years later, in 1932, by the School in Baghdad. It will be noted that this was still well before our own Institute in London was founded and it is significant that one of the stated aims of the Jerusalem School was to train personnel for the Department of Antiquities of the Palestine Mandatory Government. Shortly after the Second World War, in 1948, the British Institute of Archaeology at Ankara was opened, and then, after another gap of thirteen years, in 1961 the British Institute of Persian Studies. Finally we must note the British Institute for Afghan Studies, founded in 1972, and the British Institute at Amman, formally established in 1980. Each of these establishments has had, as you might imagine, its own varied history, which it is not the place to consider here. They share several characteristic features, however, which are relevant to my purpose. First, each one is, in theory at least, autonomous, with its own governing body; secondly, each one is, generally speaking, in practice if not always in theory, tied to a geographical sphere of activity dictated by modern political boundaries; and, thirdly, each one receives the bulk of its finances out of the public purse via the British Academy. Whether this is a legitimate use of public funds is a question each must answer for him or herself; in the light of my earlier remarks concerning the special importance of Near Eastern archaeology my own answer is plainly 'yes'. The problems, the strengths and weaknesses of this way of organising British Archaeology in the Near East have recently been subjected to two reviews, a personal, though public, one by David Whitehouse in his Seventh Beatrice de Cardi lecture, given in Manchester in 1982,¹ and an official, though more restricted, one carried out by the Academy itself on behalf of the DES in 1985. I shall not attempt to summarise these reviews here, and shall content myself with endorsing, as we all would, I am sure, the remarks of Dr Whitehouse when he says that 'when one combines local knowledge, a hostel, a library and a research programme one has an asset in the Schools and

Institutes which is valuable (on occasion, essential) to archaeologists and others at all levels, from the individual student to the full-scale expedition'. Valuable, certainly and, indeed, on occasion essential, though on other occasions not so essential, as those of us like David Oates and myself, who have been excavating perfectly happily in Syria for the past decade without the local facilities of a School or Institute, can testify. But it is still legitimate to ask, I think – as Dr Whitehouse himself asked – whether this is the best or only way in which British archaeological activity in these Near Eastern countries can be organised, the best or most economical way in which public funds can be used for the purpose.

Before attempting to answer that question it might be as well to comment on the state of Near Eastern archaeology today and its most essential needs. I remarked earlier that an enormous amount of information concerning the Ancient Near East had accrued over the past century or so of archaeological investigation; some of you may therefore think that little more needs to be done. Unfortunately this is not so; our information, though extensive, is severely biased in favour of traditional areas of study, such as art- and architectural-history, and is also often extremely unreliable, given the usually very poor standards of fieldwork current in the region until recently. (It was, I believe, Wheeler himself who said that more archaeological crimes had been committed in Palestine than anywhere he could think of.) Quite apart from developments in excavation method, etc., it is only within the past twenty years or so that serious attention has been paid to such matters as settlement patterns or the early environment. As Joan and David Oates have recently pointed out: 'The blanks on the [Near Eastern] archaeological map are far greater in extent than those small regions that have to some extent been filled in . . . Near Eastern archaeologists still lack basic pottery sequences over hundreds of years, and those sequences that do exist do not permit the identification of contemporaneity so vital to the proper application of geographical models'.² One can fully endorse their conclusion that the need in the Near East is still for 'basic, primary, exploration'.

How shall we go about satisfying that basic need? There are, I imagine, two things on which all archaeologists will agree. The first is that we should all have more money for our research and the second that we are not likely to get it. We must then, clearly, make sure that the little money that is available for Near Eastern research is spent to the best purpose. This brings me back to the point I raised a moment ago concerning the role of the Schools and Institutes. Without wishing in any way to denigrate the enormous benefits which these Institutions have bestowed and hopefully will continue to bestow on Near Eastern archaeology, I venture to suggest that they suffer from a certain inflexibility and even parochialism, largely because of their semi-autonomy and their geo-political restrictions. The financial resources which are within their control are not, therefore, always available for those projects or those regions which might, on a wider view of the Near Eastern archaeological scene, be considered to be of the greatest priority. 'Priority', I know, is a contentious word, and it would be difficult to imagine any two Near Eastern archaeologists immediately agreeing on any order of priorities when it comes to financing fieldwork. However, in a period of contracting resources it is imperative, in my opinion, that we grasp the nettle and at least consider an overall strategy for the Near East. David Whitehouse made a somewhat similar appeal five years ago, when he suggested that 'we reconsider our various programmes of research to see

whether we could usefully pool some of our resources in major international projects'.³ Although I have myself certain reservations about international projects, my own experience of them in the Near East having not been entirely happy, I confess that I, at least, was disappointed that the Academy's review of the Schools and Institutes did not, apparently, consider the possibility of some degree of pooling or redeployment of British resources in the area.

Any mention of an overall strategy, particularly if it is linked with centralised funding, must carry with it overtones of 'interventionism', initially distasteful to many of us. Almost forty years ago Mortimer Wheeler addressed this problem in his Inaugural Lecture,⁴ when he, too, asked the question 'how should research be organised?' 'This is not an easy question to answer,' he admitted, but he went on to say that, on balance, and with reservations, he would 'incline to systematic co-ordination of effort'. 'On all sides', he continued, 'we are hedged in by irrelevant restrictions and immediacies. Our best fieldworkers are inhibited by lack of funds, by lack of labour, by geographical or political iron curtains . . . Chance and salvage govern our labours; we stand in imminent risk of losing our powers of long-term thinking, of effective generalship. We need a plan'. His words are, I believe, as apposite to Near Eastern archaeology today as they were to Roman archaeology in 1948; we do indeed need a plan. In fact, I think the need is even greater today than it was then, since with the proliferation of research interests and methods, particularly of methods derived from the natural sciences, and with an ever increasing amount of recondite information to be digested, we are in constant danger of becoming more and more engrossed in our own specialisms, thereby losing the all-important overview. We are losing sight not only of the wood but also of the trees and seeing little more than the branches and the leaves. There is a serious danger, I believe, of our being tempted to promote a line of research simply because it intrigues us, or because it is technically possible, or fashionable, regardless of its archaeological value. The creation within the College of the AAWS committee should, of course, lead to a breakdown of specialist boundaries within archaeology as a whole, and the Sumerian Agricultural Project, involving members of our own Department of Human Environment amongst others, shows how successfully new fields of research in the Near East are being tilled. But examples could also be cited of less well-designed co-operation, where all relevant aspects of the subject have not been sufficiently explored. More could be done, more usefully, if we had an agreed plan for the Near East as a whole. I am reminded that, at the time Wheeler was speaking, the CBA was producing its own *Survey and Policy of Field Research in the Archaeology of Great Britain*,⁵ and I wonder whether something along these lines should not now be considered for the ancient Near East. The Schools and Institutes themselves are too constrained by local loyalties and conditions to do so; it is perhaps a task which the new Association of Near Eastern Archaeologists, currently in process of being founded at the instigation of Nicholas Postgate and Charles Burney, might care to take up. Such a survey, such a plan, should, in my view, not be concerned in the first instance with local Near Eastern political conditions or with the legitimate policies and priorities of local antiquities authorities and archaeology departments, although these, of course, will have to be considered when a strategy is implemented. In the first instance the strategy should be drawn up on strictly academic grounds and should seek to establish a

consensus as to what are the most important questions worth answering concerning the ancient Near East, and what methods can best be employed to answer them: large scale multi-disciplinary excavations; small scale 'problem-orientated' soundings; intensive surface surveys; rapid extensive reconnaissance; geo-archaeological projects; ethnoarchaeological projects; laboratory investigation of certain categories of data, etc. All have a part to play, but each has to know its place, there must be no duplication of effort, and the results envisaged must justify the cost and the effort. In short – we need a plan.

You would not want me at this late hour to enter into a lengthy discussion of how such a plan could be implemented. Nevertheless, there are a few relevant aspects which I would like to mention briefly, particularly as this will enable me to lead you back to Bloomsbury in order to consider the part that the Western Asiatic Department of the Institute and the College, as well as Near Eastern Departments in other Universities, can and should play in such implementation. I have already drawn attention to the fact that the Institute, *qua* Institute, has in the past played a relatively minor role in field research in the region, and this is by and large true of other universities. There are, I believe, reasons for thinking that a greater university involvement in Near Eastern field archaeology would have beneficial effects and I would now like to explore these.

As a practising field archaeologist working in the Near East, I am constantly aware of three features of the present archaeological situation there. In the first place, I am aware of the relatively large number of talented young – and not so young – people from this country available to work there and eager to do so, most of them trained in British universities, many of them in this building. Most of them, of course, have no permanent employment. Secondly, I am aware of the fragile and uncertain basis on which most field operations are financed. By this I do not only or even primarily mean the level of funding, though clearly that level could in almost all cases be increased with advantage. I mean rather the way in which funds have to be raised with monotonous regularity each year for each new season of work, involving a multiplicity of application forms, (each one, incidentally, slightly different from the next), testimonials, and committee meetings, with which many of you will be as familiar as I am. And, thirdly, I am only too well aware of the fact that very few of the results of all our enterprise ever reach the light of day within an acceptable period of time, if at all. In this connection I will only remind you of what I said earlier in this lecture, that much of Petrie's excavated material has still not properly been published. It seems to me that there is a connection between these three problems – a reservoir of unused talent, a precarious financial basis, and a failure to publish – and that it should not be beyond the scope of our ingenuity to devise a common remedy which might go some way to solving them.

A clue to such a remedy might lie in what to me seems to be an inherent weakness in the way field archaeology is organised abroad through the Schools and Institutes, and not primarily through the relevant university departments. I refer to the difficulty, often the impossibility, of keeping the research team together at the end of the field season. The outstanding record of our own Field Unit is surely due in no small measure to the fact that it comprises a closely-knit team of people working more-or-less throughout the year on the material from the sites investigated over relatively short periods in the field. It is true, of

course, that the few permanent members of staff at the Schools abroad have local facilities – storage and laboratory space and a certain amount of scientific equipment – even sometimes a budget which includes provision for research assistants. But this applies only to major projects in countries where such Schools exist; there are many smaller, yet still important, pieces of fieldwork being carried out, often by younger scholars without university affiliation, though with the blessing and support of the Academy and the Schools. Many of you will have experienced the sense of anticlimax at the end of each short season of such a project: the money has run out, specialist staff fly back to their jobs, students disperse to their lecture rooms and dole queues and packing cases full of valuable research material await the cheapest mode of transport home. If the Director is lucky he will have an attic where he can store the material when it arrives home and a wife whom he can bully into drawing it. He may be fortunate in finding another research grant with which he can continue to work on the results of the season and perhaps employ a helper or two, but much of his time will be spent on raising money for the next field season. It is a depressing, hand-to-mouth way in which to carry out research which we claim to be important, though it has been with us for a long time; even Kathleen Kenyon could only prepare the Jericho material for publication because she had a large shed in her garden and enough private income to employ a research assistant.

Contrast this with a team research project in a university science department – even a science-based archaeology department. Unlike the Academy, the Research Councils will often award grants for up to three years at a time, thus enabling the project director to devise a proper strategy for his work, to select a team which he can reasonably expect to keep together for more than the few months of data-collection in the field and over which he will retain a close degree of day-to-day control. His university department will provide laboratory and storage space as well as the necessary equipment – never as much as he would like, of course, but some. In return – and this is a very important point – his departmental colleagues and students will benefit from close contact with his research, and his research assistants will often make an important contribution to departmental teaching. More money is involved in all this, naturally; but it is my contention that a much more economical and cost-effective use of the little money available today for field archaeology in the Near East – and elsewhere abroad too, I have no doubt – would result if a similar attitude were to be adopted towards field archaeology as is to laboratory based aspects of the subject. We hear a lot these days about ‘science based archaeology’; according to Professor Harris, as quoted last week by the *Times Higher Education Supplement*, this is the direction in which the discipline is set to move in the future. If this results in a consequent cutting adrift of archaeology from its humanistic roots, I for one will deplore it. But if we can all agree that archaeology, to be a successful field of research, has often to adopt the methods of the ‘hard’ sciences, let it not be forgotten that an excavation is, in a very real sense, a scientific experiment, involving team work and sometimes expensive equipment and logistic support far from the home base. It also requires laboratory equipment and staff facilities at home, at the end of the field season, as I have explained and it is here, I believe, that university departments could play a larger role. What I am suggesting is that, once we have agreed on an order of priorities – a plan – for Near Eastern archaeology, the Academy, the Schools and Institutes Abroad, and the universities should act co-operatively to

begin its implementation. Fieldwork could, I believe, in many cases be with advantage jointly sponsored by university departments and the Schools. The members of the field team – including the Director, if he is not already a university teacher – could be temporarily attached to a department, which would attempt to provide the 'well-found laboratory' which is one half of the dual support research system. In return, research grants from the funding bodies, including the Academy, could be channelled not only through the Schools but through departments as well – hopefully for more than one year at a time. There would remain many problems; but I am convinced that only by some such greater co-operation in purpose and co-ordination of effort shall we be able to provide more employment for the available talent, to implement our research strategies effectively and to ensure essential publication of the results.

Ladies and gentlemen, I have already taken several steps into the Beyond – perhaps even into the world of fantasy. If this lecture has become something of a party political broadcast on behalf of Near Eastern archaeology, so be it. I can think of no aspect of archaeology more important than that of the Near East, in all its diversity, and few as important. Yet it is a subject which is under threat throughout the United Kingdom and is particularly endangered in this University. Its greatest need is for more and better fieldwork. There is no end to the opportunities available, not only in the traditional countries where British Schools and Institutes exist, but also, and more excitingly, in areas such as Syria and the Arabian Peninsula where work has hardly begun. The Institute, and the constellation of sister departments in UCL which it has newly joined, must grasp these opportunities before it is too late.

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Contextual Archaeology: An Interpretation of Catal Hüyük and a Discussion of the Origins of Agriculture

by IAN HODDER

Archaeologists have always claimed that it was easier to reconstruct past economies and technologies than past symbolic meanings. Despite Collingwood's influence on the discipline, his historical idealism (Collingwood, 1946) has most frequently been countered by a sceptical empiricism. Most archaeologists feel that it is all too easy for different people to read different thoughts and meanings into the past. For example, what appears to be a painting of a fat horse with an arrow-like mark on the side in a Palaeolithic cave in south-western France can be interpreted in a number of ways. It can be seen as a pregnant wild mare with arrows directed at its body. Or it can represent a pregnant horse associated with a branch, the two together indicating late spring (calving time). Or it is a well fed horse and both the horse and linear signs (a stylised penis) are in the category 'male'. Or all horses represent a female principle.

Such interpretations must necessarily derive from our own experiences of the world in which we live, influenced to some degree by ethnographic and historical knowledge. But the main problem is 'how do we decide which interpretation or which group of interpretations are correct?' One response to this question is to put one's faith in cross-cultural universals. For example, it might be claimed that straight linear signs are always 'male' rather than 'female'. While cross-cultural laws about, for example, processes of bone decay may have some validity, it is less easy to argue persuasively for universal meanings for material culture symbols.

An alternative approach, to be preferred here, argues that all universal assumptions have to be discussed critically, involving an evaluation of the relationship between a past context and a present context. In the Palaeolithic art example, this procedure would involve first, questioning why we assume today that linear signs are male, and second, searching for associations in the past between the linear signs and men. Unfortunately, the second aspect of this contextual archaeology (Hodder, 1986) is impossible to take very far in relation to Upper Palaeolithic cave art. There is insufficient information about settlements and burials to allow associations between men and a particular class of signs to be identified.

Contextual archaeology depends on good data. It depends on having sufficient links between various aspects of the recovered information. It needs a richly networked symbolic system. In order to demonstrate the feasibility of, and methods for, the reconstruction of prehistoric symbolism using a contextual approach, the exceptionally rich data from Catal

Hüyük will be used. Mellart's (1967) description of the art, houses, burials, figurines and pottery from this site provide a rare opportunity for interpretation to be grounded in specific contextual linkages.

Catal Hüyük

The interpretation of Neolithic (late seventh millennium BC) symbolism at Catal Hüyük (levels X to II) is facilitated by Mellaart's (1966; 1967) own interest in and development of this theme. Indeed, his discussions of this site and Hacilar (Mellaart, 1970) involve components of both contextualism and structuralism (as in his opposition of red and white, positive and negative, useful and harmful, life and death in the Hacilar pottery decoration, *ibid.*, p. 129). Much of my own account will therefore be less a re-interpretation of Catal Hüyük than a demonstration of the contextual methods used by Mellaart. However, as noted above, the contextual approach also involves consideration of the analyst's own context in relation to the past. The taken-for-granted assumptions which are imposed from the present on to the past need to be exposed. All interpretation involves making such assumptions but, as the discipline develops, older assumptions are critically evaluated.

In Mellaart's Catal Hüyük interpretation, as in much archaeology, the assumed separation of the ritual from the mundane recalls the absolute distinctions between church and state, public ceremony and private daily life, that are made in present-day western societies. The implications of this assumption in the Catal Hüyük case are far reaching and we need to 'deconstruct' this taken-for-granted before proceeding with a contextual account. Certain structures at Catal Hüyük are larger and have more evidence of ritual and plaster and painted decoration. The dead buried in these structures are generally better provided for (Mellaart, 1967, p. 207). Such structures are therefore called 'shrines', in contrast to the less decorated 'houses'. The excavated portion of the site has so many 'shrines' that it is described as a 'priests' quarter'. In many small-scale societies, such as the Nuba (Hodder, 1982), the ritual and domestic spheres are not separated. Mellaart's assumption about a separation between the two domains of activity is not a necessary one, neither is it strongly supported by the Catal Hüyük data. Both 'houses' and 'shrines' have the same internal organisation, they both have hearths and ovens, they both have storerooms and both are kept relatively clean without food debris. Both have burial beneath the platforms in the north and east parts of the structures. There is a continuum of variation from 'houses' which are small with little ritual elaboration, to 'houses' which have bucraenia, to 'shrines' with burials without grave gifts, to 'shrines' with much elaboration and 'rich' burials. There is also a spatial mixing of 'houses' and 'shrines' in the excavated portion of the site. It is difficult to argue, therefore, that the 'shrines' did not have domestic functions and that they were not inhabited in ways comparable to the 'houses'. While differences between structures clearly do exist, all the structures will here be assumed to be 'houses' or domestic units.

Another taken-for-granted under which the archaeologist working in the eastern Mediterranean labours, is the notion of the Mother Goddess (see, for example, Crawford, 1957). It has become acceptable to interpret human representations in this light, even when the sexual attributes of a particular representation are unclear. At Catal Hüyük there are many

wall reliefs showing human-like figures with arms and legs outstretched. Unlike many of the statuette figurines which are clearly female, the illustrations published of the figures in the wall reliefs do not show clear female breasts or genitalia. The relief figures are often said to be giving birth to animals and such an identification supports the Mother Goddess interpretation. But a human form (of uncertain sex) depicted above the head of a bovid need not be giving birth and the outstretched arms and hands need not express a birth-giving posture. Overall the associations of these humanoid reliefs are too ambiguous to allow interpretation as women or mothers, still less as goddesses.¹

Women, death and houses

In searching for a part of the symbolic network which is less ambiguously interlinked, we can start with supposed depictions of female breasts containing vulture skulls at Catal Hüyük. Within the contextual approach, where every symbol is assumed to be related to others in a relational whole, the point of entry into the context is arbitrary. Wherever one starts, the end returns to the beginning. In practice, however, the archaeologist is constrained by the available data. Many leads will be found to go nowhere. As will be shown, the breast-vulture link does lead into a rich network of relationships.

Mellaart's identification of protuberances on the Catal Hüyük walls as breasts containing the beaks of vultures has been accepted by others (e.g., Cauvin, 1972; Todd, 1976). But does it stand up to critical evaluation in relation to contextual data? I will argue here that it does. On the wall in house VI B 'a pair of pendulous woman's breasts appeared, open at the nipple end and painted red like a muzzle of the bull. From each of them the beak of a Griffon Vulture (*Gyps vulvus*) protruded, as each breast contained a complete skull' (Mellaart, 1967, p. 126). In house VII 35 there are two pairs of breasts on the walls. From the open nipples of one protrude the teeth of a fox, and from the other a weasel's skull (*ibid*, p. 106). On the east wall in house VII 21 is a large ram's head 'placed beneath a bold clay horn from which a single breast protrudes. Out of the open breast springs the lower jaw of a gigantic boar with formidable tusks' (*ibid*, pp. 106–7). In house VI A there is a row of boars' jaws covered by clay protuberances which Mellaart interprets as breasts (*ibid*, p. 129).

What evidence is there for the interpretation of the clay protuberances in all these instances as breasts? It is easy to impose interpretations from outside without adequate consideration of alternative 'internal' or 'emic' views. For example, the small mounds of clay may have functioned simply to hide the small skulls and jaws. Indeed, the 'breasts' sometimes occur singly or in rows, without a pairing; where there is pairing it is sometimes vertical, with one 'breast' placed below another. In general, however, the 'breast' interpretation does appear reasonable. At first sight the illustrated breasts on the house walls look rather unlikely. They have open ends and sometimes appear rather too elongated. Yet figurines of undoubtedly women show clearly that breasts are sometimes depicted at Catal Hüyük as elongated with an open nipple (*ibid*). This contextual link, in addition to the pairing of the protuberances and the high frequency of female depictions in the figurine material at Catal Hüyük, lend credence to the 'breast' interpretation.

So far, then, women's breasts are associated with vulture beaks, fox teeth, a weasel's

skull and boar tusks. There seems to be a common theme running through these associations, to do with scavenging, the wild, danger, predation and death. Such links would be further supported if one could accept Mellaart's interpretations of goddess figures giving birth to bucrania. 'From the abdomen of the right-hand body an enormous bull's head, nearly 61 cms. in width, with powerful horns protruded' (*ibid*, p. 122). But as already stated, there is insufficient evidence in the published illustrations for such interpretations to be readily accepted (see, however, note 1). The figurine material does, nevertheless, show a close association between nude women and felines, probably leopards. Women are shown holding leopard cubs and sitting beside or on leopards. Such evidence again links women with (presumably) wild predators. At the same time, the figurines show women with children and with emphasised breasts. Thus women are not only associated with death and predation. If we follow through the association between women and vultures, we are lead again to death. Cauvin (1972, p. 64) accepts the link between vulture beaks and breasts because of a further association between vultures and women. A white marble, unambiguously female figure from house VI A25 was found together with the figure of a bird of prey, probably a vulture, and the statue of an individual of uncertain sex, interpreted by Mellaart as an adolescent male (Mellaart, 1967, pls 80, 82).

If vultures show some connection with women, they also show close connections to death. Wall paintings of vultures occur in three houses in levels VII and VIII at Catal Hüyük. In one case the vulture has the legs of a human being. 'The bird represented is probably the Griffon Vulture (*Gyps Vulvus*) of Anatolia, very frequently seen in the Konya Plain' where they eat dead flesh (*ibid*, p. 168). In the wall paintings the vultures are associated with headless human figures, often lying contracted on their left sides. These scenes can be linked to the Catal Hüyük burial ritual in four ways. First, the separation of the head from the body is evidenced by the human skulls placed on the floors in the buildings. Indeed, it is particularly in the houses painted with vultures that such skulls are found (*ibid*, p. 84). Other wall paintings are interpreted by Mellaart as showing groups of skulls or decomposing heads (*ibid*, p. 168). Second, most of the burials which occur at Catal Hüyük beneath platforms within the houses, occur contracted on their left sides. Third, there is evidence from the disarticulation of many of the bones, for excarnation prior to burial. Vultures would not have removed heads from human bodies, but they could have been involved in removing the flesh, at least figuratively. Fourth, wall painting scenes dealing with death and vultures are placed above platforms on the north and east sides of the houses, below which the dead were buried. Male burials occur beneath the platform in the north-east part of the house, women and children are found buried beneath the platform set against the middle of the east wall.

In following through the network of links suggested by the initial association of female breasts with vulture skulls, a path has led to the associations of human burial. In particular, there is a close link between death and the domestic unit. Burial occurs beneath the platforms along the north and east walls of the domestic unit. It does not occur in storerooms, courtyards or outside the settlement. Individual graves are rare and later burials frequently disturb earlier ones. One house contained the bones from 32 individuals.

There is then, in general, a close association between houses and death, whether in

terms of burial beneath the floors, depictions of death on the walls, or the teeth, skulls, bears and tusks of wild animals embedded in the walls. This set of relationships also includes women, perhaps particularly the reproductive aspects of women. Even if the identification of the protuberances on the walls as breasts is rejected, the figurine material supports a close link between women and predators. The link between certain types of predator and death is also clear.

The house-death-women complex seems to allude to associations and oppositions of various kinds. For example, women appear as both reproductive and destructive. Are women to be associated with death and men with life, or the other way round? Is the inside of the house to be linked to death and the outside to life, or the other way round? How does the wild-tamed opposition fit in? These questions are imbued with expectations of a rigid symbolic code. It can be argued, however, that in most societies, symbolic oppositions are used strategically in relation to contextual factors (Hodder, 1986; Miller, 1985). Thus women can mean both death and life, from different perspectives, at different moments and in different contexts. While the analyst can recover the 'underlying' schemes, the bringing of them into play is a creative social act with multiple dimensions of meaning. The next task in relation to the Catal Hüyük data is thus to isolate the various dimensions of meaning (the underlying oppositional schemes) in relation to the house-death-women complex. After that has been achieved, the social strategies that create change through time can be considered.

Male-female

It might be expected that one dimension of the house-death-women complex is an opposition between the cultural categories male and female. Such an opposition is shown in the burial data. Of the adult skeletons, or parts of skeletons recovered from the house floors and identified by Angel (1971), 84 were male and 132 were female. Women were found buried with obsidian mirrors, personal ornament and cosmetic sets, including shells filled with red ochre. Men were found buried with much less personal adornment, but with weapons, including maces, daggers, knives, arrowheads and a few sickle blades.

Never buried with the dead are pottery vessels or figurines of humans and animals. This separation of clay artefacts and human representation from the burial context leads to another female-male contrast. Female clay and stone statuettes are much more common than male examples. In fact no male statuettes are known after level VI. Mellaart further argues that women are shown in the statuettes associated with children and leopards, and men with bulls. Without the advantage of being able to examine the statuettes themselves, it is difficult to have confidence in either the identification of the men or the bulls. The men do not have clear sexual characteristics although they sometimes have beards. Whether or not Mellaart's interpretation of the smaller number of 'male' statuettes is accepted, the overall emphasis on female depictions in this domain is clear.

After level VI, most of the statuettes (all female) are made of clay. Conversely, none of the supposedly male statuettes below that level are made of clay, all being made of various types of stone. As already noted, it has been necessary to discount the interpretation of the humanoid plaster reliefs on the walls as female. Nevertheless, it is of interest that wall

paintings showing live human figures, women and animals are rare. These things appear instead in clay as figurines. Rather, the paintings show men, often hunting with bows and arrows, perhaps in some cases wearing leopard skins (*ibid*, p. 160). The paintings also show geometric designs, hands and landscapes.

Men and women are thus treated differently at death and they are represented differently in different cultural domains. There is a clear link between men and hunting, both in the wall paintings and the burials. This association, to which I will return later, sets up a contrast between the wall decoration associations of women with the teeth, tusks and beaks of wild animals and birds, and the burial associations of men with weapons. To some extent, therefore, the male-female opposition cross-cuts any opposition that can be made between wild and domestic.

Wild-domestic

The faunal evidence from Catal Hüyük shows that domesticated cattle formed a major part of the animal economy in level VI and there is circumstantial evidence for cattle domestication in levels X to XII (Perkins, 1969). The wild fauna includes deer, sheep, fox, wolf and wild boar. By level VI, fourteen domesticated crops are present, occurring in great quantities in the upper levels.

As already noted, the burial associations link men to hunting, as do the wall paintings depicting men hunting aurochs. Indeed, much of the symbolism at Catal Hüyük relates to wild animals and cattle. There are many bucrania in the houses and depictions of leopards on the walls and in the figurine material. Given the quantities of domesticated plant material recovered from the site, it is therefore particularly significant that agriculture plays little or no role in the symbolism. Ears of wheat and barley are rarely depicted (Mellaart, 1967, p. 163 and personal communication). Storage areas do not seem to be major foci of symbolic expression. If pottery was used in relation to grain and food storage and preparation, it is of interest that pottery was not painted until right at the end of the Catal Hüyük sequence. Out of 200 rooms excavated, only one sickle was recovered.

The only clear association with grain is that female statuettes were found in a heap of grain in house VI A44 and in a grain bin in a level II house. Given the separation of women from weapons in the burials, it is tempting to follow Mellaart in linking men with hunting and women with agriculture. In any case, the contribution of the agricultural domestic side is clearly down-played in the surviving symbolism. The main arena for the negotiation of social interests is clearly the symbolism of animals and the wild. Yet part of this negotiation may have been to negate or mask the contribution of agriculture (perhaps linked to women) and domestic production.

Inside-outside

The discussion of the domestic-wild dimension of meaning does not immediately appear to have much connection with any inside-outside dichotomy at Catal since, as already shown, the insides of the houses contain much symbolism from the wild. However, within the houses a very clear dichotomy emerges between the southern undecorated hearth and oven area, and

the northern area containing burials, bucraenia and wall reliefs and paintings. Entering the house down the ladder one always would have arrived in the southern area. Indeed, the position and arrangement of this 'kitchen' area and the position of the female burial platform show the least variation between houses. That food preparation in the southern part of the house is not found with symbolic elaboration, in contrast to the elaborate northern part of the house where so much of the decoration is linked to the wild, again expresses the domestic-wild dimension of meaning. Once again, as was suggested for the undecorated pottery and the lack of grain symbolism, the domestic food production side is down-played.

The opposition between wild and domestic is contained within the house, lending support to the partial linking of the symbolic elaboration at Catal Hüyük to within-family, male-female negotiation. This linkage is also seen in the relative location of male and female burials. Along the eastern wall of the house, the female burial area occurs to the south of the male burial area. The female burial area and the adjacent hearth area in the southern part of the house are the most 'fixed' areas in the houses. The positions of other functions in the houses vary, but not the southerly hearth and female burial areas, which are less elaborate and less linked to the 'wild'.

That the house unit itself, in addition to the settlement as a whole, is an important scale for social analysis is shown not only by the considerable variability between houses and by the importance of symbolic elaboration on the inside rather than on the outside of the house walls, but also by the organisation of refuse. The houses were kept very clean and broken animal bones, for example, are rarely found within the house walls. It is the courtyards immediately outside the house walls that were used for refuse and sanitation (Mellaart, 1967, p. 68). Burials do not occur in the courtyards, neither does evidence of cooking and baking (*ibid*, p. 68). These contrasts, and the fact that dirt was allowed to build up immediately outside the houses, suggest that a major boundary between inside and outside was at the house wall itself with the house roof as an intermediary domain. The focus is placed on the relationships between men and women (and perhaps also between individuals of different ages) within the domestic unit.

Life-death

Within the domestic unit the opposition between the elaborate, 'wild', inner and to some extent 'male' northern part of the house and the undecorated, outer, and 'female' southern hearth area is itself linked to the life-death dimension of meaning. Burial is only found inside the house and only towards the inner, northern end. That the separation of life from death is a significant one is clear from the evidence of excarnation (the removal of flesh) and the removal of skulls. In some houses human skulls occur placed on the floor and scenes of death and burial occur on the walls.

An association between death and the wild is suggested by the vulture paintings. But what about the association of death and vultures with women?

Putting the pieces together

As more and more dimensions of meaning are considered, it becomes less easy to identify any clear structure of the following type:

men:women::life:death::domestic:wild

This structuralist type of argument would necessitate disregarding contextual variations. Within any one context, and from a particular perspective, women may be equated with the life-giving properties. However, in other instances, and from other perspectives, they may be linked to death. The women–house–death complex with which the analysis of dimensions of meaning began, has had to be broken down. It has become more diverse, less of a ‘whole’, as the individual dimensions of meaning are examined.

For example, there is evidence to support a link between women and clay in that it is mainly in this medium that women are depicted. We have also seen that figurines and clay pots do not occur in burials. This separation of the female from death is also seen in the spatial divisions, south and north within the house. The depictions of women with children (figurines), of breasts and of women giving birth (Mellaart’s interpretation of one of the statuettes) could be interpreted as supporting a link between women and life, in opposition to death. But we have seen many other links which tie women securely to death. Similarly, women can be associated with inside as opposed to outside, with domestic as opposed to wild. Yet there are just as many links between women and the wild outside and between men and the most inner parts of the house. As a final example of the complex relationship between the various dimensions of meaning, it is not even clear that inside–outside correlates with domestic–wild in all contexts. There is much that is ‘wild’ on the inside – the innermost part of the house is steeped in symbolic references to the outside world.

In putting the pieces together we should not expect a rigid code, good for all contexts. The analysis should not expect clear correspondences between the dimensions of meaning regardless of context. The same point can be argued on theoretical and ethnographic grounds (Hodder, 1986). The dimensions of meaning do not determine all action within a social ‘whole’. Rather they are drawn upon in the active creation of social life.

It is only within specific contexts, defined potentially in many cross-cutting ways, that some repetition and consistency are to be expected. Thus, within the statuette material, it is the case that men are rarely depicted, decreasingly with time. Men are always associated with one aspect of the wild – with hunting. Women, on the other hand, are associated with another aspect of the wild – with predators and scavengers. All aspects of what might be expected to be female productive tasks in the domestic sphere are under-represented in the symbolic elaboration. In the organisation of space within the domestic context the female burial platform and the southern hearth area are fixed, the pivots around which other things vary. But in wall paintings, reliefs, pottery decoration and other symbolic representation, the female role in production and the contribution of domestic production are down-played and made peripheral.

Thus it is not possible to argue that the women–house–death complex is built up from a simple set of societal-wide rules. Rather, it has been possible to identify several dimensions of meaning that are significant at Catal Hüyük by searching for oppositions within specific aspects of the materials. Each context of action, however, is not a mirror image of other contexts. The oppositions do not always line up in the same direction. Each context is used as part of the social negotiation of cultural meanings. The task of the analyst is not to crack a unitary code, but (a) to identify the dimensions of meaning used in a particular society, and (b)

to discover their social locus. The pieces cannot be put back into a whole, but they can be located in relation to the social process.

Social strategies

The main dimensions of meaning identified at Catal Hüyük concern the oppositions between male and female, wild and domestic, life and death. The main social locus at which negotiation in these terms takes place would appear to be the relationships between men and women. In particular, Mellaart (1967) is frequently drawn to the two-sided nature of female representations as both wild and domestic, linked to both life and death.

Women are depicted nude, with their sexuality clearly portrayed. Mellaart claims that they are frequently shown giving birth, although doubt has been expressed here (see note 1 however). Women are also shown with predators and scavengers from the wild, although such a relationship is not necessarily negative. For example, women are shown holding and perhaps caring for leopard cubs, but the associations between women, vultures (and hence death, as suggested by the wall-paintings) and the teeth of predators and scavengers are clearly of a different type.

These two aspects of female representation (reproductive and death-like or dangerous) would have had marked social implications. The importance of women and domestic production was already effectively denied in many contexts (as seen in the undecorated hearth area in the house, the undecorated pottery and the paucity of agricultural symbolism). Yet, as reproducers, they created the domestic unit of production and reproduction on which male strategies would have depended. It is the world of animals and the male domain of hunting in the wild that is symbolically emphasised. Female production and reproduction are frequently denied or made dangerous.

This type of social interpretation appears to account well for the bringing into conjunction of male-female, life-death, domestic-wild dimensions of meaning. It could also account for the linked inside-outside dichotomy. The locus of symbolic elaboration is the house, and the main boundary between inside and outside occurs at the house wall. It is within the house that men and women have to be both brought together (within the domestic unit of production and reproduction) and kept apart (since the importance of women in production and reproduction has to be denied or made dangerous). Especially since there is evidence of social production at a larger scale, the house and the domestic arena become the stage for the use of the life-death, domestic-wild oppositions in male-female strategies.

All symbols have a potential to be 'read' in different, often contradictory ways. By exploring the varied contextual associations of a range of symbols at Catal Hüyük, we can begin to understand the multiple ideas that a particular symbol might have evoked. For example, a statuette of a nude woman, with open nipples, associated with leopards, could have been 'read' to emphasise the woman as reproductive, and 'lording' over the wild; dangerous yet powerful. On the other hand, the dangers of the wild, the associations of breasts with vultures and of women with death, could have involved exclusion and lack of power. In the same way, the female burial platform and the domestic hearth and oven area in the southern part of the house act as fixed points, central to the organisation of the house. Spatially they are

central but in the lack of decoration on the walls in the southern part of the house, and the lack of pottery decoration, the area of domestic production appears subordinate. In terms of decoration, it is peripheral.

The varied dimensions of meaning identified at Catal Hüyük can be seen to be involved most closely in the negotiations between men and women within the domestic context. Women were represented, mis-represented or made invisible in different domains. The ambiguous and contradictory meanings of symbols were involved both in establishing women as life-givers and life-takers. The dependence of society on women is incorporated, transformed and denied. Yet what are the long-term consequences of these strategies, and how are they changed through time?

Change through time, the origins of agriculture, and Hacilar

The economy at Catal Hüyük involves domesticated plants and animals in addition to hunting and gathering. Catal Hüyük occurs early in the process of domestication and there appear to be close links between that process and the symbolic schemes identified above. Domestication would potentially affect any symbolism linked to the wild, especially when it is considered that hunted aurochs became domesticated cattle in Europe and the Near East. The word 'domestication' itself points to the house and the domestic sphere.

It is not adequate to describe the cultural and symbolic spheres as just dealing with economic and social events as they happen. It can be argued that most theories for the origins of agriculture have not successfully explained the originality and creativity of the acts involved. In the published accounts which emphasise population increase, environment, economic or social relations in a systemic perspective, one is always left wondering why solutions other than agriculture were not adopted. The creation of domestication, the directionality and the driving force are not adequately accounted for. Cauvin (1978), however, suggests a rather different explanation for the origins of agriculture which not only puts symbolic and cultural factors as prime movers, but also provides reasons for the generation of change from within the human symbolic capacity. Human beings adapt not to nature, but to nature as perceived. As we have seen, at Catal Hüyük, nature and the wild were involved in a complex social-symbolic scheme. Cauvin (*ibid.*, pp. 116–17) notes that, in the Near East, bull symbolism occurs prior to cattle domestication. He suggests that a large aurochs, as well as representing half a ton of meat on the hoof, was above all a dangerous force, appropriate for representing and evoking a thousand irrational fears and insecurities. The natural world, in Cauvin's account, becomes something other than a neutral spectacle or objective provision. It is also a problem internal to the subject. Great emotional and psychological forces create new solutions as individuals try to deal with their fears. It is at the locus of these fears that creativity will be concentrated and change will occur. Certainly specific conditions encourage or are appropriate for the development of agriculture, such as a sufficiently high population density, social cohesion, perhaps economic pressures. The driving force is not the need to tame the external world, but the need to tame the world within us.

It is not only psychological problems which are involved in the taming process. The 'wild within us' incorporates social divisions within society, as the Catal Hüyük evidence suggests.

The moment of reproduction is itself linked to death. It has been easiest to discuss such fears in relation to male-female strategies. Yet, because of the association of the fundamental sexual pairing with danger and death, it is conceivable that many of the social relationships based on reproductive links, or on the circulation of men and women, were similarly imbued with avoidance, danger, fear and death. The taming of the wild is thus intimately connected to the ability to change society and also to control it.

The process of domestication is thus broader than has traditionally been supposed in archaeology. It involved not only the domestication of plants and animals; most of the changes generally linked to the Neolithic Revolution can be seen to be connected with 'culturing' the wild. The building of more stable houses, the aggregation and even delimitation of settlement, the more elaborate and cultural treatment of the dead, all separate more securely the domestic from the wild. Pottery production transforms natural clay into cultural ceramic and the decoration of vessel surfaces makes cultural that which the vessels contain. Much of the symbolism at Catal Hüyük is reminiscent of that found more widely in the Near East immediately prior to and after plant and animal domestication (Cauvin, 1972; 1978). Within this symbolism we find that domestication also involves taming the wild dangers associated with death, reproduction and female sexuality. In seeking explanations for domestication we are not justified in dealing only with the economic. Rather the plant and animal changes are part of a wider process in which cultural control is expanded and nature dominated.

Today nature is transformed into a cultural product for social use by objective technological processes. I have tried to show that for the so-called 'origins of agriculture' the transformation of nature into culture involved psychological, social and symbolic factors which may well have played dominant roles. But the overall patterns of increasing 'culturing' of the environment, the greater need to provide a symbolic domain within the culturally constructed rather than within the natural world, the greater emphasis on control and domination through culture that occur from the ninth to the fifth millennium BC in the Near East and Europe are of such a force and magnitude that even those factors considered here seem inadequate. All I have tried to achieve in this paper is an awareness of the problem, as suggested by the data from Catal Hüyük.

Catal Hüyük itself covers a time span of approximately seven hundred years. How are the dimensions of meaning (male-female, wild-domestic, death-life) involved in social strategies through time? Can these changes be seen to continue at Hacilar which covers the following seven hundred years within the sixth millennium BC?

As expected from the general hypothesis of increased domestication in all spheres, the importance of 'the wild' decreases through time at Catal Hüyük. Spearheads and arrowheads become smaller after level V and there is a marked decline in the chipped stone industry in levels III and II. Large horns used as bucraenia are found in the lower levels (VII and VI) but they later decrease in size. This is all rather tangential evidence for Mellaart's (1967, p. 224) suggestion of the gradual 'hunting out' of the Konya Plain (see Angel, 1971). More important for the present discussion is the apparent decrease in the value or prestige of hunting seen in the decrease in size and quality of hunting weapons. With a decrease in the social-symbolic importance of hunting, one would expect to see similar decreases in the importance of the

symbolic dimensions with which it is linked, particularly on the wild–domestic axis. Indeed, there is a decreasing amount of wall decoration through time, although this may be linked to less good preservation in the upper levels. More clearly, however, plaster reliefs do not occur after level V. The emphasis on the house as a boundary between wild and domestic diminishes.

By the end of Catal Hüyük, therefore, the wild–domestic axis has come to play a less significant role in the household. It is thus of interest that at Hacilar the houses are not decorated. In addition, burials do not occur in the houses, but apparently outside the settlement. In fact, the ‘back’ area of the Catal Hüyük houses is absent at Hacilar. For example, in Hacilar VI, the whole undecorated house is organised around the hearth and oven, reminiscent of the southern undecorated hearth area of the Catal Hüyük houses. The death, the wall paintings, reliefs and bucraenia have gone. Rather than being entered from the roof, as at Catal Hüyük, the Hacilar houses are more ‘open’, with doors in side walls. In this new scheme it is difficult to identify the old oppositions so clearly. By level I at Hacilar, statuettes appear with the dead as well as in the houses, breaking down another opposition noted at Catal Hüyük. There is no excarnation at Hacilar.

This is not to argue that the old scheme entirely disappeared. At Hacilar all the figurines are female and the women are shown either dressed in leopard skins or seated on or holding leopards. Such figurines are not found in the upper levels at Hacilar. As well as the decrease in importance of the link between women and predators, the locus of symbolic elaboration changes. Pottery begins to be painted in the upper level at Catal Hüyük, but painted pottery becomes common at Hacilar. Mellaart (1970) suggests that the designs on the pottery at Hacilar evoke the house symbolism at Catal Hüyük. Interpretation of most of the Hacilar pottery designs is difficult, but bucraenia are certainly present. Some pots clearly depict human heads. It is quite conceivable that many of the same concerns over wild–domestic, life–death were now focussed on pottery decoration. Male–female may also have been involved. At Catal Hüyük we have seen evidence of an association between clay and women, continued in the clay figurines of women at Hacilar. In addition, the painted pots are found at Hacilar clustered around the hearth and ovens, as are the female figurines. The pots may therefore have been especially linked to women.

There is no ear of corn shown in the Hacilar symbolism. Female figurines again occur embedded in grain. The importance of agriculture and the female as producer remain largely hidden and women remain represented in the figurine material, and perhaps in the pottery, as reproductive and dangerous, especially in the earlier levels at Hacilar. Also, at the same time, many of these dangerous connotations are pushed away from the house, outside the settlement. Hunting is less important and burial is extramural. By level II, Hacilar is surrounded by a wall. A more completely cultural arena is created within the settlement. As a result, new social relations can be mapped out and a different form of society constructed based on social group production rather than individual unit production.

This move towards socialised production is seen at Hacilar in that ‘kitchens’ are added on the outside walls of the houses, and ovens and hearths appear in courtyards (unlike Catal Hüyük). Separate granaries also occur (Mellaart, 1970, p. 29) and there is a suggestion of centralised pottery production. Together with the decreased symbolic elaboration of the

house and the house walls, and the appearance of a wall around the whole settlement in Hacilar II, the evidence suggests greater integration of the larger group. To decorate the insides of the houses now becomes redundant as the main social strategies occur at a larger scale than the domestic unit. Pottery, on the other hand, is decorated. Mellaart suggests that the Hacilar painted pots were not used for cooking. They would have been clearly visible around the hearths and ovens inside and outside the houses and would have been an ideal medium for comparison and evaluation between domestic units.

The domestication of the natural world, perhaps partly born of a desire to arrest internal fears, implies an erosion of the central importance of the wild in the symbolic constitution of social life. But this 'culturing' process was long, emotive and difficult and the violence in the imagery of Catal Hüyük marks its passage. The two-sided nature of female sexuality was always to remain, but its centrality declined as hunting and the wild declined. Figurines, hunting and images of the wild gradually disappear together.

As the wild and the fears it brought to the social world are tamed, the way was open for new social games to be played, involving more elaborate social relations, social co-operation and control and, ultimately, increasing social hierarchy. On this new stage, the old dimensions of meaning involving life-death, wild-domestic, male-female, played new roles. At Hacilar we see life-death linked to inside-outside, now at the level of the larger social group. The old meanings are used to create a new society concerned with its relations as a whole to the outside and with its relationships with the non-social. The old images are reused to compete and to compare within the larger unit of socialised production.

Conclusion

The style in which this paper is written does nothing to help us understand its content. The dry, objectivist and classificatory tone of all scientific discourse distances us from subjective experience. As a scientist and a scholar I have not been able adequately to capture the psychological and emotive dimensions of the Catal Hüyük material. Mellaart is so much more successful, both in his freer writing style and in his use of reconstructions of houses and of the rituals that took place in them. However inaccurate in detail, the drawings that he publishes of dark rooms with their skulls, wall paintings and bucrania surely capture some of the attributes of the subjective experience of life at Catal Hüyük.

It is only when we recognise the richness and power of the emotions involved in such domestic rituals that the dry hypotheses suggested in this paper to explain the symbolism at Catal Hüyük, and to redefine the 'origins of agriculture' debate, come to have any force. I have argued that cultural and symbolic processes may not be secondary or tertiary in relation to the economic and social spheres. In fact the process of domestication involves more than plants and animals, economic and social relations. Existing theories concerning the so-called 'origins of agriculture' have inadequately dealt with other dimensions of the domestication process, such as the 'domestication' of clay, houses, death and female reproduction. I have further suggested that part of the exploration for this process lies in a human desire to tame deep-rooted psychological fears, to domesticate the wild within us and within society, and to

reduce the wild to a distant metaphor. I am aware that this is not a full or adequate explanation, but it does seem to lead in new directions which may prove to be fruitful.

This paper began as a contextual analysis of a specific body of material. It seems important initially to try and comprehend sites like Catal Hüyük in all their 'otherness'. Of course there are many weak links in the chain of argument where the Catal Hüyük data are insufficiently networked. I have intentionally allowed the discussion to build up from the particular to the general because it is only in this way that the relevance of the general to the particular can be adequately assessed. At the general level I have argued that the domestication of plants and animals, at least in the Near East and Europe, must be placed within the context of related 'domestication' in other domains. Further discussion of the validity of the Catal Hüyük reconstruction will depend on analyses of other sites within a broader context.

Notes

1. Many of the wall reliefs remain unpublished. This additional information may throw light on the female and birth-giving aspects of the human-like plaster reliefs. Indeed, Mellaart suggests (personal communication) that the many fragmentary reliefs and paintings do show a child or animal head appearing from between the widespread legs of undoubtedly females. The figures are identified as female on the basis of, for example, depictions of swollen abdomen, vulva, menstruation, breasts, facial features, long black tresses, female attire, and bonnets (personal communication). Such evidence emphasises the two-sided nature of the female metaphor at Catal Hüyük, as both life-giving and life-taking.

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Iron Age Britain and the Continent: The Contribution of the Institute

by BARRY CUNLIFFE

The archaeological world of the 1930s into which the Institute was born was, for Iron Age studies, a world focused on hillforts. Pitt Rivers had shown the way in the 1870s with his examination of Sussex sites, and others had soon followed – the Cunningtons in Wiltshire, the Curwens in Sussex and Christopher Hawkes in Hampshire. Excavation then was on a very restricted scale commensurate with the limited objectives of contemporary scholarship – the desire to establish a cultural sequence and to construct a quasi-historical model by means of which British sites could be related to supposed movements of people from the Continent. It was also a time of stocktaking. The excavation of St Catherine's Hill, near Winchester, from 1925 to 1928 provided Hawkes with the opportunity to review some 30 years of hillfort digging (Hawkes *et al.*, 1930) and in a seminal paper published a year later the picture of the British Iron Age, as it was then conceived, was blocked out with broad brush strokes (Hawkes, 1931). In the same year, in collaboration with Gerald Dunning, he laid the second foundation of Iron Age studies in 'The Belgae of Gaul and Britain'. The 1930s opened, therefore, in an atmosphere of satisfied assurance – the structure was there and all that was needed was to fill in the details. In terms of excavation strategy little had changed and hillforts continued to be subjected to small-scale sampling.

It was into this world that Mortimer Wheeler was inevitably drawn. His interest in what we would now call cultural interfaces, particularly Roman/barbarian interactions, developed in Wales and was further enhanced by the seasons he spent excavating the native and Roman town of Verulamium. Hillforts, now so much in vogue, presented the next logical area within which he could pursue his interests and parade his remarkable organisational skills. But his work at Verulamium had taught him that if significant advances were to be made (and he was not a man to waste his time simply finding more of the same) he would need to mount his campaign on a grand scale.

By 1934 he had settled on Maiden Castle and in his introduction to the report (Wheeler, 1943: 1) he sets out his research aims, commenting on the large number of hillforts in Wessex:

Of these sites, a number, notably in Wiltshire, had been 'sampled' to a greater or less extent, and substantive work had been carried out in the east at Hengistbury Head and in the west at Hembury Fort; but in the great central area, where Maiden Castle is the outstanding monument, no methodical work on any considerable scale had yet been

attempted. A large and important cultural province thus remained unsystematised, and much miscellaneous material found here and there within its borders was devoid of scientific context. The problem was one which found a natural focus in the great Dorset earthwork. . .

but significantly he gives as his first reason for choosing Maiden Castle that,

although not the largest in area of British earthworks the huge and involved defences of the site have long been recognised as the most imposing of their class; and it was felt that the time had come when this prestige should rest upon a wider range of values than those inherent in complexity and magnitude alone.

Here indeed, was a stage grand enough for the master!

It was typical of Wheeler that he should choose a massive complex like Maiden Castle to match his ego but it was equally typical that he had the vision to see that Iron Age studies were in a doldrum – work had hitherto been far too piecemeal and small scale. New questions had to be posed and new research designs created to confront them if knowledge of the Iron Age was to advance. It was this vision and sense of scale that he brought to the Institute, and with it he enthused his associates – colleagues and students alike.

There is no need for us to spend time considering the achievements (and shortcomings) of Wheeler's Maiden Castle project. Suffice it to say that the excavation design showed a brilliant economy, enabling a maximum amount of structural and sequential information to be extracted with the minimum of effort. But perhaps more important, the excavation provided a training ground for a whole generation of young professionals many of whom were to remain in close association with the Institute throughout its early years.

During the Maiden Castle campaign Wheeler developed the theory that the major structural changes which turned the univallate hillfort (Maiden Castle A) into a massively developed multivallate structure (Maiden Castle B) were the result of an incursion of Veneti fleeing their Armorican homeland in 56 BC at the time of Caesar's conquest. Later, he argued, in AD 25 the final C phase began with the advance of Belgic tribes from eastern Britain. As the theory formed in his mind so his interest in Brittany and Normandy increased and by 1936 he had decided to mount a major programme of survey and sample excavation in north-western France. The context and rationale are precisely given in his statement to the Leverhulme Trust to whom he applied for funds:

The subject of research for which the Grant is asked is the further investigation of the origins, in northern France, of the later prehistoric civilisations of Britain. This piece of research has a special reference to the civilisations associated with the building of the very large number of fortified hill-towns or 'camps', particularly in southern England, during the last three centuries before the beginning of the historical era.

Considerable work at Maiden Castle, Dorset, and elsewhere has been carried out in recent years into this remarkable development of urban life in Britain on the eve of the Roman occupation, and it has become increasingly clear that further progress in the reconstruction of the archaeological and historical factors involved must include some

investigation of equivalent remains in western Normandy and Brittany. In these regions little work has been done, or is likely at present to be done, by French investigators, but in a preliminary exploration of the material with special reference to our British problems we are already assured of the co-operation of our French colleagues.

The difficulty is now financial. Whilst it is possible nowadays to raise funds in this country for archaeological work at home, there is no likelihood for raising equivalent funds for extending that work (as it should be extended) across the Channel. Hence the present application.

The work, as envisaged, will involve a general ground-survey of the later prehistoric hill-towns and earthworks in the regions named, together with such trial-excavations (carried out on quite a small scale) as shall be necessary to determine the nature of the cultures represented.

It is submitted that this piece of work is one which is calculated to enlarge substantially our historical horizon in respect of those early cities which have the distinction of standing at the head of the whole tradition of urban life in this country. The moment is on general grounds a peculiarly opportune one in relation to the development of prehistoric studies in this country; and a recent summary review of the French material has indicated that, with a grant of £250 a year for two years, a very considerable advance of knowledge should be feasible upon the lines indicated.¹

The memorandum has been quoted in full both for its historical interest and to serve as a model for us all in how to write intelligible and persuasive grant applications!

In both the planning and the execution of the programme Wheeler relied heavily on his co-workers, in particular Kitty Richardson and Ralegh Radford (Plate 1). Radford was involved in much of the original fieldwork and provided crucial advice during the planning stage. In a letter written to Radford, then at the British School at Rome, on 5 January 1937, Wheeler acknowledges Radford's suggestions:

Meantime, I have slightly re-orientated my outlook on the situation. The pottery most nearly analogous to our south-western hill-fort stuff (bead rim, etc.) seems to come from western Morbihan and Finistere, and I have veered round to your original view that sondages at Huelgoat would be the best preliminary step – especially since the change of proprietorship at Le Petit Celland is likely to involve delay and difficulty. It may be necessary of course later on to try the Petit Celland and perhaps two other sites in Normandy and the Loire basin; but in any case, it will be useful either positively or negatively to know whether our people had any sort of affinity with the occupants of what is pretty clearly the biggest hill-fort of central or western Brittany.

In order to save time, therefore, and in view of your own instinctive preference for Huelgoat, I have written to Lantier and to Schaeffer putting Huelgoat first and Le Petit Celland second. I hope you will approve.

¹Wheeler's correspondence, from which various quotations are taken in this paper, is in the care of the Society of Antiquaries by whose permission it is here used.



Plate 1 Wheeler with Claude Schaeffer, Ralegh Radford and Kitty Richardson at Camp d'Artus, Huelgoat, Finistere.

The results of the French campaigns are well known from the report which was to appear nearly 20 years later (Wheeler and Richardson, 1957) and from Jacquetta Hawkes's recent account of the intriguing social context within which they were mounted (Hawkes, J., 1982: 179–95). Both accounts leave one in little doubt that the work was highly organised but only by reading the file of administrative correspondence which survives is it possible to appreciate Wheeler's organisational skills and keen attention to detail. Nothing was left to chance: before any major involvement the ground was carefully prepared. In March 1938 an expeditionary force comprising Kitty Richardson, Meg Whitley and E. S. M. Perowne, was sent out to pave the way for the excavation of Camp d'Artus near Huelgoat in Finistere (Plate 2). In a letter dated 24 March Wheeler writes to Perowne giving details of the travel arrangements and itemising what he would like done. The first instruction was to visit the hillfort at Le Petit Celland near Avranches, find out who the owner was and obtain permission to excavate. Then

To go to Huelgoat, Finistere, taking with you the official permit from the Ministre des Beaux-Arts for trial excavations in the Camp d'Artus, which is in a wood 1 km to the



Plate 2 Work in progress at Camp d'Artus, Huelgoat. The kind of scene that Wheeler would not have chosen to illustrate his report.

north of Huelgoat. This permit I will give to you before you go. At Huelgoat, the following things are required:

- (a) To interview the Prefect of Police and to warn him of our arrival at the end of July, and generally to establish friendly relations with him.
- (b) To see the local Inspecteur des Forêts and to tell him ditto. (The forest in which the camp is situated belongs to the Département des Baux et Forêts.)
- (c) To book accommodation at one or other or both of the little hotels at Huelgoat, and to find out what facilities there are for accommodation in local pensions places. At the hotels (of which I believe the better is the Hotel de France) rooms will be wanted, at a date which I will confirm before you start, for myself, for you, I hope, for Theodora Newbould and for Kitty and Meg (unless they prefer otherwise), whilst some sort of cheaper accommodation will be required for my foreman, Wedlake, and my photographer, Cookson [Plate 3]. The best thing, however, will be to obtain a list of hotels and



Plate 3 Wheeler's photographer, Maurice Cookson ('Cookie'), was known for his sense of humour. Here, presumably, he is making an ethnic point.

pensions with prices and to do the booking at the end. We must try and keep the prices as low as may be.

If you can between you do all this important preliminary work, I cannot say how tremendously useful you will be to the whole expedition and how materially you will help forward its work.

By June all was ready and in another flurry of correspondence we find Wheeler deeply involved in last minute arrangements. On 30 June he writes to his foreman Bill Wedlake:

My dear Will,

I am shortly sending you a ticket from Camerton to France for the night of Tuesday, July 26th. You should be at Southampton in time to get to the St. Malo boat by 8.0 p.m. I will meet you on the boat.

I take it that you have arranged about your passport? I am also sending £1 for preliminary expenses.

Yours in haste,

It is against this background of personal involvement at all levels and minute attention to detail that Wheeler's success as a leader can perhaps best be understood.

Nor were the niceties of good public relations overlooked. A report on the 1938 season was to be presented as a paper to the Society of Antiquaries, then meeting in the evening, on 23 February 1939. Beforehand Wheeler organised a dinner for his financial backers and the influential archaeologists of the day. That they were all male allowed him to call the occasion a stag party and this point was taken up by Alexander Keiller in his letter of acceptance:

Only the other day I read an article in an American Police Magazine entitled *I Strip at Stags*, which proved to be a form of diary of one of the army of damsels who specialise on dancing, and generally entertaining, on such an occasion. I shall look forward, consequently, with lively anticipation to your party, and not less so in view of the fact that you have selected the somewhat unexpected venue of the Athenaeum for the purpose.

The aftermath of the Maiden Castle excavation and the French campaigns were inextricably bound up with the birth of the Institute. The energy, sense of pioneering and academic anticipation which surrounded the work added enormously to the excitement of the nascent organisation: Wheeler's Iron Age research programme instilled a feeling of purpose and direction. Academically the work was outstanding because of the scale of the questions asked and the vision employed in their answering. While Wheeler was prepared to work broadly within the 'historical' framework established by Hawkes, modifying it when he felt it necessary, his methods were innovative. He was determined to drag what was fast becoming a parochial and moribund discipline across a new threshold. The *Maiden Castle* report, in many of its aspects, still serves as a model today, while *Hillforts of Northern France* remains a prime source for French Iron Age studies and, in spite of a flurry of activity in recent years, is unsurpassed in quality and range.

Purpose, vision and scale: these qualities characterised Wheeler's work of the 1930s – the high peak of his achievements. To the generation of helpers attracted to him, the sense of urgency and direction he instilled must have been irresistible. Not surprisingly many of those who were involved with him in this creative decade took up Iron Age studies, forming a bridge spanning well into the 1950s. Ralegh Radford, Molly Cotton, Kitty Richardson, Kathleen Kenyon, Leslie Murray Threipland, John Ward Perkins, Thalassa Henken and Meg Whitley – these names crop up time and time again in the archaeological literature of the 1940s and 1950s. Indeed, it would be no exaggeration to say that the vast bulk of the new material on the Iron Age, published during this period, was generated by the Wheeler-inspired generation.

Perhaps the strongest link (and surely one of the strongest personalities) in the story was Kathleen Kenyon. As the first secretary of the Institute in the pre-war era she remained on the staff until 1962 when she left for Oxford. Kenyon had worked with Wheeler at Verulamium and though not directly involved in the Maiden Castle campaign or the work in France, she developed a deep interest in Iron Age Britain excavating at the hillforts of Wrekin in 1939, Breedon-on-the-Hill in 1946 and Sutton Walls from 1948 to 1951. Of all the Wheeler protégées Kenyon was the one who took the most academic interest in Iron Age studies. By 1950 she was finding Hawkes' ABC scheme, published 20 years before, to be in need of

extensive revision and in the Institute's eighth annual report (1952) she published a wide-ranging critique under the title 'A survey of the evidence concerning the chronology and origins of Iron Age A in Southern and Midland Britain' – 50 pages of careful analysis laced with good sense. The main thrust of her argument was inspired by Wheeler's remark, 'The only safe general classification of Iron Age A in Britain is on a geographical, not a chronological basis' (Wheeler, 1943: 186). After reviewing the ceramic evidence she concluded that the case had now been made 'to show that local characteristics in the forms of Iron Age A pottery in different parts of the country are sufficient to suggest regional cultural groups' (Kenyon, 1952: 76). This was a major step forward and, while not welcome in some circles, pointed the way forward. The paper was to be Kenyon's swansong in Iron Age studies for she now turned her considerable energies to different themes.

By now the Institute was growing and in 1955 Sheppard Frere was appointed to teach Iron Age and Roman studies. Frere's international reputation is firmly based on his contribution to Roman archaeology but he had, in his early career, developed a keen interest in Iron Age matters and was to maintain this throughout his time at the Institute (1955–66). His work at Canterbury and his influential large-scale excavations at Verulamium required a detailed understanding of the Iron Age background. It was out of this that his interest in Iron Age coinage developed and, in close collaboration with Derek Allen, the Index of Celtic Coins was born. (It has now grown to become an archive of outstanding importance.) Frere used his detailed knowledge of Celtic coinage to help sketch the opening chapter of *Britannia*, his textbook on the history of Roman Britain published in 1967, the year after his move to Oxford.

Nor was his fieldwork restricted to purely Roman matters. In 1957 and 1959, in tandem with Molly Cotton and Alwyn Brogan, he led a small contingent of Institute students to Gaul to excavate the hillfort of Le Charlot in Corrèze and to carry out survey work in the region (Cotton and Frere, 1961). A few years later, again with Molly Cotton, a useful excavation was mounted on the Buckinghamshire hillfort of Ivinghoe Beacon for three seasons from 1963 to 1965 (Cotton and Frere, 1968).

With its long tradition of Iron Age studies and Frere's active involvement in these matters, the Institute was the natural home for the CBA's conference on 'Problems of the Iron Age in Southern Britain' held in December 1958. Judging from the published proceedings, which Frere was to edit, the conference was an outstanding success. A great deal that was new was aired, including an updating of the ABC scheme, by Christopher Hawkes, and a fascinating paper by Derek Allen, entitled 'The Origins of Coinage in Britain: A Reappraisal', supported by a full gazetteer of Celtic coins from Britain. There was also a thoughtful contribution from Sheppard Frere himself exploring a number of problems posed by the later Iron Age including a critical assessment of Wheeler's theory of Venetic immigration in 56 BC – the pivotal date of the Maiden Castle chronology – which Frere convincingly showed to be no longer acceptable.

The conference proved to be a turning point in Iron Age studies. In some ways it was oddly old fashioned, basking in the comfort of established dogma and yet the signs of disquiet were there. The decks had been cleared for action, uncertainties had been expressed and the study was at last beginning to lose its innocence.

The revised ABC scheme which Christopher Hawkes first put forward at the conference was published in the next year's *Antiquity* (Hawkes, 1959) in advance of the appearance of the conference proceedings. It was in essence a complex three dimensional framework, based on the original model of 1931, extended and refashioned to contain 30 years of new work. For those brought up with the old scheme it was reassuring but to the new generation the scheme imposed unacceptable constraints on creative thought. These views were cogently and forcefully expressed by Roy Hodson (who had joined the Institute staff in 1959) in a now-classic paper 'Reflections on "The ABC of the British Iron Age"' (Hodson, 1960). He begins by admitting that 'it may seem surprising for a newcomer to question an approach that was generally accepted for 30 years and that has been recently revised and re-acclaimed', but then goes on to say:

After all, the 'New ABC' is most unusual in its approach: to name all Iron Age groups after a closed series of three letters, and not after an ever-expanding series of type-sites, to define cultural boundaries by fixed geographical limits and not by the concordant distributions of type-fossils, and to fit these groups into an absolute, not a relative framework, all this seems quite contrary to accepted practice and to the methods so successfully employed and so clearly described by Childe. The ABC scheme implies that the British Iron Age is something apart, not amenable to normal archaeological methods. But is this really so?

In the remainder of the critique he highlights a number of specific problems and then goes on to outline the way forward: 'The first step should be to define groups in their own right, without immediately interpreting their significance or encumbering them with continental labels. . . . A second requirement would be some form of relative chronology.' Finally, he asks, 'Perhaps it is time for another Iron Age Conference?' It was the opening salvo – matters were not to rest there. Over the next few years Hodson was busy putting flesh on the skeleton. Two years later he published a detailed consideration of pottery from Sussex and its broader context (Hodson, 1962), in 1964 extending the study still further with a survey of cultural groupings within the British pre-Roman Iron Age (1964a) and an even more sweeping review of La Tène chronology (1964b). Once more he returns to the practicalities of making a real advance:

The essential steps in establishing a British chronology for material from settlements, hillforts, etc., with which this study is largely concerned would seem to be (1) a series of detailed local relative chronologies (not typologies!) that could be (2) linked to an inter-regional relative chronology for Britain, that could be (3) related to the continental relative system, which may be (4) progressively modified and given absolute dates (Hodson, 1964a: 99).

In these four devastating papers, appearing in quick succession, Roy Hodson totally redirected Iron Age studies in Britain. For those of us who were beginning to explore the field these were exciting times. The new-found freedom instilled a sense of purpose. It is no

exaggeration to say that Hodson's work provided an inspiration equal, in its own way, to that of Wheeler's pioneering efforts nearly 30 years before.

In the last 20 years the Institute has continued to produce work of high quality, serving both as a research base for postgraduate research students and also as a forum for a continuous stream of influential lectures, many of which continue to be published in the Institute's *Bulletin*. To select individual contributions for specific comment would be invidious, not least because of the short time perspective available. Instead let us briefly review the progress made in two of the themes which have absorbed the energies of members of the Institute past and present.

We have seen that one of Wheeler's prime interests lay in the relationship of the communities of Armorica with those of central southern and south-western Britain. He was impressed, as others have been since, with the apparent cultural similarities between them, particularly those exhibited between the western extremities of the two territories. Cliff castles and souterrains are common to both while the small defended settlements which his teams surveyed in Armorica (but which he never published) will have reminded him of the Cornish rounds and the raths of south-western Wales. In the multivallation of the Wessex hillforts and the developing use of the sling he saw the direct influence of refugees fleeing from Caesar's devastating Armorican campaigns, while similarities in pottery, particularly internally grooved rims and countersunk lugs, he considered to be strong supporting evidence. Frere's contribution at the 1958 conference, reviewing Wheeler's model, cast serious doubts upon its chronological viability particularly in the light of the then current dating of the Le Catillon hoard (Frere, 1961), but there matters were allowed to rest as a problem demanding further attention.

In the new critical mood of the early 1960s those of us who had just come to work in the field saw nothing for it but to discard existing models and to follow Hodson's advice – to begin again with the material in an attempt to reconstruct cultural groupings and sequences free of preconceptions. Eventually the day after day spent in museum basements studying pottery and other material, much of which had remained unpacked for 30 or 40 years, began to produce results. Several of the more important clues had, however, already been published by Wheeler in 1957 in his report of the French expeditions. At Le Petit Celland he had discovered a distinctive black cordoned ware in contexts which could reasonably be assigned to the Caesarian campaigns or a little before; this he realised was identical to pottery found in quantity at Hengistbury by Bushe-Fox in 1910–11 and hitherto thought to be several centuries earlier. Clearly an explanation was needed:

It was not for nothing that Hampshire and the Manche (with the Channel Islands) confront each other across the Channel. Furthermore, the complete absence of the Hengistbury bowls (so far as is known) in their undevolved form elsewhere in Britain suggests only a momentary cross-Channel contact; and, although there is an easy risk of overworking the possibility of associating such contacts with the rigorous campaigns of Julius Caesar in north-western Gaul, it is by no means unlikely that these bowls, together with another class of 'foreign' Hengistbury pottery noted in connexion with

Kercaradec, are the relics of a few ship-loads of refugees at the time of the Caesarian conquest. They may, on the other hand, be commercial products of a somewhat earlier date (Wheeler and Richardson, 1957: 47).

While he was still reluctant to let go of his refugee hypothesis, in that last sentence Wheeler was opening up an entirely new range of explanations.

It was David Peacock's pioneering work on imported amphorae (Peacock, 1971) that finally presented the problem in sharp focus. Considerable quantities of the Italic Dressel 1A amphorae, datable to the late second and early first centuries BC, were found at Hengistbury showing that the site had, for a brief period, been a port of entry for foreign goods carried by ships along the Atlantic seaways. It was in this context that the Armorican pottery probably reached central southern Britain.

Since then several advances have been made. An extended programme of excavation at Hengistbury has shown the importance of the site as a port-of-trade importing wine, figs and coloured glass (and no doubt much more besides) and exporting British-produced raw materials such as gold, bronze, iron, corn and possibly even the hides, slaves and hunting dogs mentioned by Strabo as exports from the island in a slightly later context (Cunliffe, 1987: especially 339–43). Meanwhile our French colleagues have been particularly active in Brittany showing that comparable sites existed at St Servin (Langouët, 1984) and Quimper (Le Bihan, 1984), while Italic amphorae, particularly the Dressel 1A type, were imported to Armorica in considerable quantity (Galliou, 1982).

The model which we can now put forward, based on the evidence of the last ten years of research, is of an active but possibly quite short-lived *contact period* when Atlantic trade flourished and Mediterranean luxury goods were available for the first time in quantity. The most likely context for this sudden flowering is the impetus given by Roman entrepreneurial activity working out of the newly acquired Provincia Transalpina in the period between annexation c. 120 BC and Caesar's rapid advance through Gaul 60 years later. Against this background the possibility of Armorican refugees using the cross-Channel routes to escape from Caesar's wrath sinks into insignificance.

But what of the period before c. 120 BC? It is inconceivable that Brittany and south-western Britain were not in regular contact, particularly in the light of the interest in Cornish tin to which several classical sources refer. The apparent escalation of trade in the *contact period* was probably nothing more than an intensification of traditional systems. Perhaps it was in this earlier phase that the cousinly cultural relationships developed between the two areas: there is clearly much to be done in this field. What is tolerably clear, however, is that the multivallation experienced by the Wessex hillforts, and the appearance of the cultural complex which Wheeler called Maiden Castle B, is far more likely to be the result of indigenous, and probably isolated, development than of immigration. But these are broader questions we cannot hope to tackle here.

That our current views differ from those of Wheeler is of little consequence: he firmly focused our attention on a series of fascinating questions and started us in the right direction, Frere steered us away from error and Hodson showed us how to rediscover our bearings.

The second area of interest to have attracted attention from Institute staff and students in the last 50 years has been 'the Belgic problem' or more correctly the relationship between south-eastern Britain and the Continent from the late second century BC to the time of the Roman conquest. Simply stated the question revolves around how to relate Caesar's statement, that the Belgae first raided Britain and then settled, with the archaeological evidence.

Hawkes and Dunning (1931) had set out an elegant model, bringing together the views of earlier scholars and a range of new evidence, in which they suggested that the first Belgic wave arrived in the south-east of the country in 75 BC carrying with them coinage, a new burial ritual involving cremation, and a range of fine pottery. A little later, in the 40s, a second invasion led by Commius landed somewhere in the Solent region.

This general view was widely accepted, at least in outline, but Allen's reappraisal of Belgic coinage (1961) suggested that there had, in fact, been a series of six separate influxes of coins from Belgic Gaul. This suggestion was enthusiastically embraced by Hawkes in his paper 'New Thoughts on the Belgae' (Hawkes, 1968) in which he related the different groups of Gallo-Belgic coinage to successive incomings of people. But there were problems. Ann Birchall, a student at the Institute, had been studying the Belgic question for her doctoral dissertation and had failed to find convincing evidence for any burials of Belgic type in Britain pre-dating Caesar's campaigns (Birchall, 1963, 1965). This did not, of course, disprove an early Belgic invasion but it weakened one of the props used to support the traditional hypothesis. Similar problems were experienced when artefacts such as imported metal vessels and brooch types were critically examined – nothing distinctively pre-Caesarian could be recognised. Several ingenious suggestions were made to attempt to explain away the problem (Harding, 1974: 201–26; Rodwell, 1976) but none was convincing and Professor Hachmann's lecture on 'The problems of the Belgae seen from the continent', given here at the Institute in 1975, neatly summed up the dilemma (Hachmann, 1976).

Standing back from the great mass of data that has been assembled and worked over, it is simplest to accept that the constituent elements of the Aylesford–Swarling culture, as it is generally known – i.e. the 'Belgic' pottery, the burial rite and the associated metalwork – is substantially post-Caesarian and reflects the development of a socio-economic system which linked Britain to Belgica (now under Roman control) and enabled the Roman world to exploit the resources of unconquered Britain. A convenient model for such a system is a prestige goods economy of the kind so elegantly and convincingly described by Mike Rowlands and Susan Frankenstein in the *Bulletin* for 1978 with reference to south-west Germany in the sixth-fifth century BC. It is exactly the situation one might expect to find developing in south-eastern Britain after Rome had engulfed Gaul.

What then preceded it in the pre-Caesarian period? Herein lies the real problem. A few years ago when John Kent was dating the Gallo-Belgic coinage much later than Derek Allen, the answer was very little (Kent, 1981) but very recent, and still unpublished, work by Bob Van Arsdell has largely confirmed Allen's dating and we must now accept that the Thames estuary was receiving periodic supplies of gold staters from Gaul from as early as 120 BC. This would be wholly consistent with Daphne Nash's view that intense diplomatic activity linked the tribes of the Somme valley to those of the Thames (Nash, 1984) in the decades before

Caesar. Van Arsdell's study of potin coins from the Thames valley strongly suggests that these low value tin-rich bronze coins of local manufacture were being made perhaps as early as the earliest Gallo-Belgic imports (Van Arsdell, 1983), implying that already some semblance of a market economy may have been evolving.

These are matters of great interest to the specialist and I mention them here to show how fast our ideas are changing, but we are still far from locating Caesar's Belgic invaders. Perhaps it does not matter – perhaps social and economic questions are more significant. Certainly they are the legitimate and central concerns of modern archaeology. My own guess is that the Romans knew where the Belgae landed and recorded the fact when they located them in Hampshire with their Romanised capital at Venta Belgarum (Winchester) – but the arguments are tenuous and detailed research is yet to be presented (Cunliffe, 1984).

Wheeler was certainly turning his mind to Belgic matters when he extended his French campaigns to the Seine valley and upper Normandy. His excavations at the hillforts of Fécamp and Duclair were already beginning to yield interesting results when the threat of war led him to make his now-famous dash back to Britain to join the war effort. When the war was over his interests had moved on.

Wheeler brings us back to where we began. It was his vision, sense of scale and attention to detail which led to the creation of this Institute. These qualities he lavished in full measure on his Iron Age pursuits setting standards and directions which his successors here have built upon and amplified. Reviewing the achievements of the Institute and the inspiration which its staff have given to others over the last half century, Wheeler might have allowed himself his ultimate accolade of pleasure – to have bought himself a new hat.

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The Institute and Roman Archaeology: Past, Present and Future – An Outsider's View

by T. W. POTTER

It is a rather overwhelming privilege to be invited this evening [3 March 1987] to proclaim, to examine and above all, to dissect the history and achievements of the Roman Department of London University's celebrated Institute of Archaeology. For I am reminded that dissection is not a particularly easy business, and am irresistably drawn to the image of that famous nineteenth-century surgeon, Robert Liston. Liston, of course, worked before the days of proper anaesthetics and speed was therefore of the essence; and he, indeed, was speedy, so much so that in one operation to amputate a leg, he is reputed not only to have severed the offending limb, but in addition to have removed one of the patient's testicles and two of his assistant's fingers!

Now dissection is of course the very stuff of archaeology and it has been one of the jobs of the Institute to encourage and develop more precise methods than those practised by archaeologists of Liston's day, whether in the study, in the laboratory or in the field. Somewhat perversely, however, Roman archaeology was accorded little importance when the blueprint for the Institute was sketched out in those heady Wheelerian days of the later 1920s and early 1930s: perversely because the dream of an Institute which was in Wheeler's words, 'for training young archaeologists, especially in fieldwork' was born in the Welsh hills and vales at those quintessentially Roman sites of Segontium, Brecon Gaer and Caerleon.¹ This is a matter which surely demands some explanation. It is, I suspect, not wholly the consequence of Wheeler's feeling in 1934 (and here I quote from his autobiography, *Still Digging*) that 'I suffered from a satiety of Roman things. The mechanical, predictable quality of Roman craftsmanship, the advertised *humanitas* of Roman civilisation which lay always so near to brutality and corruption, fatigued and disgusted me,' and he goes on to attribute the admitted deficiencies of the Verulamium report to this sense of *ennui* with things Roman.² Wheeler's attitudes are not always easy to understand and I am bound to add that I am further deterred from the attempt by the fact that he once roundly ticked me off for running one of my Italian sites whilst in an unshaven state. Yet it seems evident to me that Wheeler shared in the view, widespread both in his day and still with us, that the Romans were a people vastly inferior to the ancient Greeks and not, in themselves, a particularly worthy area of study. Fascist philosophies that emanated from the Italy of the 1920s and 1930s did not help. Mussolini's regime, which consciously strove to emulate the imperialism of its Roman predecessor, provoked widespread revulsion in intellectual circles, which in turn was passed on to the ancient Romans

themselves. Thus D. H. Lawrence, writing in his *Etruscan Places*, published in 1932 (at the time when plans for the Institute were being drawn up): 'the Fascists . . . consider themselves in all things Roman, Roman of the Caesars, heirs of Empire and world power', and he goes on to speak of the way in which Etruscan art, once Romanised, was 'spoilt', and how the Romans 'did not like the country. In palmy days they built great villas, with barracks for slaves, out in the country. But, even so, it was easier to get rich by commerce or conquest. So the Romans gradually abandoned the land, which fell into neglect, and prepared the way for the Dark Ages'.³

Lawrence's neat summary of the decline and fall of the Roman empire would have been acceptable to many intellectuals in the 1930s. I do not doubt that it coloured the views of the founding parents of this Institute, and its legacy is still with us today. Yet the intellectual fabric underlying Roman archaeology in the 1930s was much more complex and it is worth trying to unravel some of the strands. While in Italy, and at Tripolitanian cities like Sabratha and Lepcis Magna, the Italians were laying bare what Wheeler crisply termed the 'proto Fascist glories of Imperial Rome' (the remark is an unmistakable reflection of his views), the French were doing much the same in Maghreb.⁴ Despite their total disregard for stratigraphy and, very often, of late and post-Roman structures, I admire the achievement of the excavators of vast and remote sites such as Timgad, Lambaesis and Djemila, in modern Algeria. I have profoundly unpleasant memories of a brief October season at Lambaesis, where freezing weather was little deterrent to the infinity of deadly snakes and scorpions that we continually trowelled up; I left the site with a sneaking respect for my French and, indeed, my Roman predecessors, who were tougher than me. Certainly the French saw their colonial role as closely similar to that of the Romans, as is clear from the words of archaeologists like J. Toutain: 'The better that we understand what the Romans accomplished in their African provinces,' he says, 'then the better we shall be able to direct our efforts and more speedily ensure their success'.⁵

It does not do, in my view, to underestimate the achievements of this 'Roman colonial school', as we might term it. However primitive in methodological terms, there was created a huge corpus of data, especially epigraphical; a series of remarkable excavated sites which provide astonishing insight into ancient city life; and, perhaps above all, a powerful public awareness of the Roman past. Few French *colons*, or Italian *contadini*, can have known nothing of the Romans and their archaeological legacy, however this was manipulated for political purposes in an uneasy, strifeful, modern world.

Wheeler, however, continually castigated the excavators of these sites. 'No single Roman site in France,' he wrote in 1948, in his Inaugural Lecture for this Institute, 'has yet been explored under modern scientific control'. He went on: 'Africa shows the omissions of metropolitan France . . . even great Carthage has had to content itself largely with the well-meaning attentions of amateurs. Its fated destruction [in antiquity] . . . may yet be encompassed by its modern friends'. He pointed out – and the observation is still valid – that study of the farms had, in the Maghreb, largely been sacrificed in favour of laying bare Roman cities and, dissecting as fast as the surgeon, Liston, wrote off Iberia with a savage 'the Spanish peninsula is in the same state or worse'.⁶

Writing of the state of Roman archaeology in North Africa, as he perceived it in 1948, Wheeler might well have touched upon another lacuna, namely what we could term the

Romano-African element of North African studies. We still know so little of the indigenous contribution and the less Romanised sites. Roman North Africa was seen almost exclusively from a Roman point of view. It took an article by Fergus Millar, published as recently as 1968, to stress that 'Punic was a common spoken language throughout the lifetime of Roman Africa', and that there were other linguistic groups: Berber and Libyan and, in Tripolandia, a strange script that is attested down into the fourth century AD.⁷ One has only to think, with Millar, of the Maktar *iuvenes*, with their names cited in Libyan, Punic and Latin, to appreciate the cultural mix, while I am further reminded of this by my Algerian collaborator in our Cherchel excavations, Nacera Benseddik, who was brought up to slip effortlessly from Berber into Arabic and thence into French. Such linguistic virtuosity cannot have been at all uncommon in the Roman period, although the element of the population that spoke only an indigenous tongue will also, I imagine, have been high. Here Dr Benseddik's mother-in-law comes readily to mind, a wonderful lady who, despite an adult life spent in Algiers, speaks no Arabic and no French, only Berber. I have still not forgotten my few phrases of Berber that I learnt from her in a somewhat confusing dialogue.

All this may help to explain why Marcel Benabou, a Tunisian, entitled his seminal work on the indigenous elements of Roman Africa, *La résistance africaine à la romanisation*, a pioneering volume published just over a decade ago, in 1976. Similar neglect of indigenous elements may be identified in Italy, where again it is only recently that the so-called Italic peoples (i.e. not the Romans, the Etruscans or the Greeks) have begun to receive proper attention. More than forty languages were spoken in the Italian peninsula prior to Roman unification and it is manifestly right to examine the role of this complex mosaic of people in the formation of the world's first international state. Is it not astonishing, for example, that E. T. Salmon's *Samnium and the Samnites*, published in 1967, was the very first book to treat of the culture of this formidable and impressive people?

In France, of course, the concept of not a Roman, but a Gallo-Roman culture, was established at an early stage in modern scholarship. There was the great Gallic hero, Vercoingtorix, not to mention his modern counterparts, Asterix, as well as Obelix and the bard Cacofonix. The Celtic element in Roman Gaul was rapidly defined, especially in the realms of art and religion. It was in Britain, however, that still surer foundations were laid for a proper appreciation of the nature of provincial Roman culture. Of importance was the contribution of nineteenth-century antiquarians like Charles Roach Smith (1808–90), whose magnificent collections of Romano-British material, sold to the British Museum in 1856, I now curate; here was material for study and classification, especially pottery.⁸ But it was that intellectual giant, Francis Haverfield (1860–1919), who was the first to provide an integrated picture of Roman Britain, brilliantly combining the evidence of ancient literature, epigraphy and archaeology. Haverfield held stringent views. He constantly adumbrated the importance of precise excavation and full publication – 'so much mischief has already been effected by unscientific exploration [of Hadrian's Wall]', he wrote, 'that certainty [as to its history] has perhaps been made unattainable'. He also believed in training, speaking in 1907 as follows: 'We in England have an almost disastrous conception of learning. It is not merely that we think the learned man a social nuisance or an oddity: we have a particular indifference to learning as such . . . we

. . . believe that any Englishman can go where he likes and achieve what he wants, without training and without special knowledge'.⁹

These were, I suggest, important words, redolent, as they are, with premonition. They were published posthumously in 1924 and did, I wonder, Wheeler read them, sitting on some Welsh hillside? Certainly Haverfield was concerned about the role of the Universities. 'The greatest of our archaeologists have had nothing to do with them' he observed, while, as for the British Museum, it 'cannot have the connection with University learning which, for example, supports the museums of Berlin, Paris or Vienna'.¹⁰

This is thought-provoking stuff, and so was Haverfield's brilliant attempts to define, and separate out, the Roman and the Celtic strands in the social make-up of Roman Britain. Here was a thoroughly convincing attempt to portray a province with a bilingual native aristocracy, emulating Roman ways and, at the other end of the social scale, a Celtic population living close to the land, as they always had done. I particularly like his evocation of things Celtic as exemplified by dragonesque brooches and the flowing curvilinear designs on Nene Valley pottery. When a Celtic-style brooch turned up near Frankfurt, he caustically remarked 'it so startled the local archaeologists that they assigned it to Africa'; for Haverfield, it was a vital matter to decipher the true significance of objects which, if found in other provinces, might be misinterpreted, ignored or, perhaps, even thrown away.¹¹

Haverfield's natural successor, as a giant synthesiser of things Romano-British, was that brilliant thinker, R. G. Collingwood. While Wheeler was energetically exploring Roman Wales and planning the Institute of Archaeology, Collingwood was busy collecting materials for more synthetic works: inscriptions especially, but most other classes of artefact too. He published a short general account of Roman Britain in 1924, and in 1930 his brilliant *The Archaeology of Roman Britain*, with its classification of brooches, pottery, ironwork and the like. Then in 1936 came *Roman Britain and the English Settlements*, as a magisterial overview of the Roman history of the province.

This was a book that was at once hugely influential and, at the same time, controversial. It was written in a period when many seemed to think that Romano-British archaeology was largely sorted out. When Kendrick and Hawkes published their review of the state of research in 1932, they observed that 'prehistoric studies require an interim report but our knowledge of Roman Britain has advanced some way beyond this and . . . it is actually the stage of historical revision that is setting in'.¹² Similarly, R. C. Bosanquet could elsewhere write of Hadrian's Wall that its 'difficult problems' were 'nearing solution'.¹³ Collingwood's book, couched in stylish, measured and, above all, authoritative terms, seemed to support such views. That it was in reality a shaky edifice that he had constructed was not immediately apparent. The Wheelers' much publicised Verulamium excavations of 1930–34 seemed to provide dramatic archaeological confirmation for Collingwood's view of the history of Roman towns, not least a third-century decline and, in a second edition of 1937,¹⁴ he could quote Wheeler's celebrated picture of Verulamium about AD 273 as a place that 'must have borne some resemblance to a bombarded city':¹⁵ it was a compelling image, especially as contemporary and vivid scenes from the Spanish Civil War were so much in everyone's mind.

Wheeler did himself have some criticisms of Collingwood's *magnum opus*,¹⁶ and so did

Ian Richmond. Richmond's appraisal of Collingwood, published after his death in 1943, spoke of Collingwood's tendency to 'drive the evidence too hard' and to build upon it 'a series of conclusions whose very artistry disguised the inherent weakness of foundation'.¹⁷ Richmond did not cite *Roman Britain and the English Settlements* in his bibliographies and he ignored the work in his general survey of Romano-British archaeology between 1910 and 1960.¹⁸ But Collingwood's work remained a classic, and there gradually came into being a view that Roman Britain was, in Richard Reece's delightful, and now famous words, 'a nice sand-pit in which toddlers can safely be left to play'.¹⁹ As Kathleen Kenyon remarked in her *Beginning in Archaeology*, published in 1952, 'Excavating on a Romano-British site is possibly the best training a beginner can get'.²⁰ A training for what, we might ask?

Roman, and especially Romano-British, archaeology was therefore hardly a prime area for new intellectual enquiry when the Institute was founded in 1937. Roman art and archaeology was seen as inferior to Greek, had politically unsavoury overtones and Roman Britain, in any case, appeared to be largely worked out. Besides, Wheeler was in reality much more interested in interfaces in history (though he would have shuddered at the term), and in the impact that one civilisation had upon another, whether Roman upon Celt, British upon Indian or, indeed, English upon Welsh. It was the revelation of the pre-Roman antecedents of Verulamium that had, in his words, 'enlivened' that investigation, and it was visits to Welsh strongholds like Bardsey Island that enlivened the Segontium excavations. The twelve families of Bardsey, were notoriously independent. They selected the oldest reprobate on the island as a monarch and, upon the outbreak of war in 1914, declared themselves not just a neutral power, but swore allegiance to the German Kaiser. 'Consistently,' Wheeler declares 'they took no part in the conflict until 1918, when a boatload of seasick policeman landed on the island and forcibly removed those of military age.' Wheeler, of course, made friends with the monarch of Bardsey Island on a visit in 1922 and, at the end of the day, three lively lobsters were put on board his boat by Royal Command. 'During the long and difficult journey back to Caernarfon . . . the lobsters and I', he adds 'conducted an unceasing battle on the back seat of the car. The lobster is a cunning and unprincipled fighter . . . !'²¹

I feel bound to wonder what effects those emasculating lobsters might have had upon Wheeler's thoughts on Roman archaeology as he drove back through the darkness to his excavations at Roman Segontium. At any rate, when the Institute was finally opened in April 1937, Roman archaeology was to take a back-seat role. Christopher Hawkes, in the Institute's first public lecture on 'A summary of aims and needs' could bring no great enthusiasm for it, beyond remarking prophetically that 'Hod Hill, where the Roman police fort, no less than the great Iron Age camp, should have important secrets to reveal';²² and all that really ensued was a little teaching of Roman Britain. But Molly Cotton, who was to become no mean Romanist, was one of the first students to receive a diploma; by the spring of 1945, Kathleen Kenyon, then Acting Director, was digging in Roman Southwark, in company with, amongst others, Sheppard Frere. She also gave the lectures on Roman Britain at the request of the Professor of Ancient History, and relations with the British Museum (whose Director, Sir John Forsdyke, was on the Management Committee) were forged by means of termly visits so that students could look at objects in the Reserve Collections.²³

Meanwhile, behind-the-scenes moves were afoot and the Annual Report for 1946–47 announced that the Board of Studies had recommended the appointment of a Reader and Professor in the Archaeology of the Roman Provinces. Why ‘provinces’ one might wonder, and not ‘empire’ or ‘world’? Italy, it is true, had begun to turn its back on Roman archaeology, with its pre-war Fascist association, and thenceforth Etruscology and studies of Magna Graecia were to become the popular choice of aspiring young Italian academics. Even so, it was a strange title and, in many senses, Wheeler was a strange appointment to the Chair, created in 1948. I quote Jacquetta Hawkes: ‘For four years Dr Mortimer Wheeler had been absolute ruler of the largest archaeological organisation in the five continents . . . yet, when he returned to London in the early summer of 1948 his own country offered him nothing better than a part-time professorship, which, as he said, “my friends had discovered for me”, in the Institute he had founded’.²⁴ Jacquetta Hawkes might have added that Wheeler, just back from India, had to sit with a recently created Chair of Indian Archaeology, Professor Codrington, and was, as we have seen, hardly enamoured of purely Roman studies.

For her, Wheeler’s salvation lay partly in what she terms as ‘new command in Pakistan’, and his striking exploits there are indeed a matter of history. But he nevertheless retained close touch with Roman archaeology. Molly Cotton ran a training school at Verulamium in 1949;²⁵ there were the classic excavations at Stanwick in 1951–2,²⁶ and above all there was his inaugural lecture delivered to this Institute in March 1948. It is a piece full of well-rounded and finely honed Wheelerian phrases. ‘It behoves us to take action’, we are exhorted, and he presents us with the somewhat improbable image of University Chairs being turned into Chariots! Yet much Roman archaeology received short-shrift. ‘For another 100 years, and with increasing skill’ Wheeler observes, ‘we might continue . . . to adjust the jigsaw of Hadrian’s Wall, without adding materially to our appreciation of human attainment.’ Similarly, the history of the Roman Empire might be described as ‘a rather ponderous cautionary tale in somewhat questionable taste’.²⁷ For reasons such as these Wheeler felt himself obliged to concentrate upon the people that he termed ‘the victims’ of the Roman Empire, a theme that was to culminate in his fascinating book *Rome beyond the Imperial Frontiers* (1954).

However, more mainstream Roman archaeology was initiated or supported by the Institute. Apart from the Verulamium season in 1949, in the same period Kathleen Kenyon and John Ward-Perkins were digging at Sabratha in Libya. The site was visited by Wheeler and Molly Cotton in September 1948 where, he tells us ‘there were black and tan pirates, with monstrous beards and Oxford accents’, digging amongst the ruins.²⁸ But he liked what he saw, for the work was designed to establish a chronological sequence for a spectacular and long-lived site. That the results have only just been published, due to the devoted efforts of Philip Kenrick,²⁹ would not have met with Wheeler’s approval (although he would, I feel sure, have praised Kenrick’s meticulous report); but one important outcome was the development of close relations between Libyan and British archaeologists, which, happily, continue down to this day.

The fifties were, in fact, rather exciting times in Roman archaeology. Although Chester G. Starr, writing in 1960 about the previous fifty years’ work in the Roman empire, could

account for the contribution of archaeology in just two paragraphs, lots was happening, especially in Roman Britain.³⁰ There were the excavations of the Lullingstone villa whose unique Christian wall-paintings, now a prize exhibit in the Romano-British Gallery in the British Museum, were being restored by Norman Davey in this Institute; Richmond's brilliant work at Hod Hill and Inchtuthil;³¹ and, here in London, W. F. Grimes's dramatic discoveries of the City's rich past, not least the Walbrook Mithraeum, discovered in 1954.³² Finds tumbled out of the ground, with that early post-War revelation, the Mildenhall Treasure – by then on display at the British Museum – as an incentive to further enquiry.³³

Wheeler did not, I think, encourage close links between Institute Romanists and those at the British Museum. I cannot resist quoting his notorious observation of 1954 that he 'abjured' the British Museum 'a place that suffers from a sort of spiritual cataract and outstares the visitor with unseeing eyes'. He subsequently qualified the observation in a footnote, reading 'I regret this remark. It was written before I became a Trustee of the British Museum and, had truth permitted it, I should have deleted it'.³⁴ Today, I trust, Wheeler would have wholly changed his views, and found encouragement in new attitudes and new activities.

In 1955, Wheeler retired from his part-time Professorship, having reached the age of 65. Unless I do him an injustice, he seems to have had few students of note; it is certainly true to say that he launched no notable excavation in the Institute's name while he was Professor: indeed, in the Stanwick report, published in 1954, the Institute is not even mentioned. The truth must be that the British Academy, of which he was Secretary, absorbed much of his time and interests, to the Institute's disadvantage. Even so, when the Management Committee came to appoint a successor, they felt that it was 'impossible to find a worthy successor to Sir Mortimer' and thus reverted to the original plan (framed in 1946–7) to have a Reader in Roman archaeology.³⁵

Thus arrived Sheppard Frere upon the Institute stage, with John Evans as Professor of Prehistory and W. F. Grimes, as Director, in hot pursuit. Gordon Childe, in a somewhat sad valedictory lecture as retiring Director, had spoken of the need to introduce the methods of prehistoric archaeology, such as environmental studies, into classical, Roman, even Indian archaeology: now were assembled the ingredients for change and advancement. Not that I am sure whether it is very fair to describe Professor Frere as an 'ingredient' – or, indeed, the present and past Directors – but it was certainly a prodigious cake that they baked. Amongst the slices are the sumptuous volumes on the Verulamium and Canterbury excavations, no less succulent morsels on Bignor and Dorchester-on-Thames, and that massive wedge, decorated with elegant icing, that is Frere's *Britannia, the history of Roman Britain*.³⁶

Sheppard Frere, elevated to an Institute Chair in 1963, soon attracted promising students: Joan Alcock, Ann Birchall and Bill Manning were amongst them and I discovered that a young man called Mark Hassall was on the books in 1963. The present building – that 'monstrous concrete box' as Wheeler somewhere termed it – was opened in 1958, and one senses that it was soon a hive of activity. Frere's departure for Oxford came in 1966 and it was again a case of finding a worthy successor; while Mark Hassall filled in on the teaching, the search was pursued and eventually Richard Goodchild was elected to the Chair.

I only met Goodchild once, at the British School of Rome with the Ward-Perkins, just

before he left for London in the summer of 1967. As a Mediterranean neophyte, I knew little of Goodchild's work and I found him somewhat reserved and introverted. Only when I read his collected works did I realise just how wayward my youthful judgement had been.³⁷ Here was a man who had indeed, as Kathleen Kenyon advised, learnt his trade in Roman Britain and who had gone on, with the utmost distinction, to pursue it in Libya. No-one who has worked in North Africa, whether indigenous or foreign, can be unaware of the difficulties. Goodchild had conquered most of them, performed archaeological work of outstanding quality and become a loved and respected person in his adopted country: a fitting choice, therefore, for a Chair in an Institute that both teemed with foreigners and was dedicated to training.

Goodchild's few month's at the helm – he died in February 1968 – were insufficient to achieve a really lasting mark. He did, however, implant a firmly international stamp upon the activities of the Institute's Romanists, which was to be steadily nurtured by those who followed. It was, I think, the happiest choice that Donald Strong was elected as Goodchild's successor. The archaeological world is not always noted for its harmony and I find it unsurprising that Agatha Christie found it a convenient setting for some of her murder mysteries. Think of *Murder in Mesopotamia*, where the dig director's wife was eliminated by the use of a convenient quernstone, dropped from the roof of the dig house. Now Agatha Christie was, of course, the wife of Sir Max Mallowan, who held the Institute's Chair in Western Asiatic Archaeology for many years; she therefore knew a thing or two about our world. As she remarked, via the wise Hercule Poirot, 'the state of mind of a community is always directly due to the influence of the man at the top',³⁸ and it is certainly true to say that Donald Strong exercised a most warming and fruitful influence upon his many friends, colleagues and students.

Donald Strong came to the Institute in 1968 from the Greek and Roman Department of the British Museum, thus sustaining and developing a link that had flickered and sometimes shone in the decades since 1937. He also presided over the formation of the Society for Libyan Studies, thus consolidating a connection between the two countries and, in particular, this Institute, which was again already of long-standing. He was himself soon digging in Gloucester, while Mark Hassall – by now a full lecturer – was chasing after Praxiteles' statue of Aphrodite at Knidos in Turkey, in the apt company of Professor Love of New York University. He was soon joined by adventurous students, for in 1969 the Institute launched an undergraduate degree scheme. The huge expansion in archaeology, matched by widening intellectual horizons and a spate of new discoveries, was underway. Archaeology, in David Clarke's celebrated aphorism, had lost its innocence.³⁹

Donald Strong wrote on many things: sculpture, gold and silver plate, Roman architecture and art, the Etruscans and even Eltham Palace in Kent; but nowhere more delightfully than in his Inaugural lecture, delivered to this Institute on 12 May 1970.⁴⁰ Some earlier Inaugurals puzzled him. One read like 'a recruiting poster' – no prizes for guessing which; one was 'intended to prove that archaeology was history (or it might have been vice versa)', while a third he did not understand at all.

Such remarks make it clear that Strong was the least possible stuffy man in a world which



Plate 1 Professor Donald Strong at the farewell party for Professor W. F. Grimes in June 1973 in the garden of Gordon Square. To his left is his wife Shirley, and to his right, Dr John d' A. Waechter.

could – and, I think, frequently still does – take itself much too seriously (Plate 1). He sought to illustrate his point by organising a delightful Enquiry: namely, how would the Romans have regarded the three institutes for which he had worked. The first was the then Ministry of Public Buildings and Works, now English Heritage. Strong is trying to illuminate the nature of Roman archaeology but is rather saddened to discover that ‘the Romans rather despised old buildings and ruins . . . ruined quarters of the city, however picturesque, were thought of as rather sordid!’ Restoration, in up-to-date materials, was the name of the Roman game, not record and fieldwork, while ‘buried treasure never lost its fascination’.

The curators of the British Museum, on the other hand, had their counterparts in antiquity. They may not have had museums as such, but the Romans did display treasures, very often looted from abroad, in the temples and other public places. Being a modern curator of such riches, Strong concluded was, however, probably the better deal. ‘As ancient Roman curators’, he remarks, ‘they would have been poorly paid, with an obligation to guide the public round and some distinctly unpleasant consequences for dereliction of duty’. When one of the prize possessions of the Roman State, the famous statue of a hound licking its wound in a temple on the Capitoline Hill was mislaid ‘it was enacted that the unfortunate keepers should pay for its loss with their heads’.⁴¹ I suspect that he, like me, was wondering what sort of effect such a policy might have had upon our colleagues today.

Strong’s third job was here at the Institute, and here his examining of Roman attitudes towards the Institution led him towards highly perceptive insights into the nature of Roman archaeology and its study. He makes it clear that ‘we should have immediately to eliminate the Prehistoric Department. While praising the technical perfection of their methods, the Romans (would have) shuddered at the waste of effort . . . Where are the heroes, where are the men, where are the cities?’ Here, however, he was making a very serious point, albeit in a frivolous way, about the relationship between Roman history and Roman archaeology. Let me quote further: ‘The ancient historian has miserably few facts to play with, and what facts he has have been carefully selected and predetermined for him by some good, bad and indifferent historians. So he has flattered the archaeologist into thinking that his job is to provide historical facts for the historian, which of course he isn’t . . . the danger is that we [i.e. the archaeologists] allow ourselves to be thought of as the technicians who dig and delve in the trail of the historian whether he is pursuing Tacitus across England or Caesar through Gaul’.⁴²

These words of 1970, very much anticipate – and, I think, helped to shape – a new mood of Roman archaeology, one that gathered ever-increasing momentum as the seventies wore on. Studies of Hadrian’s Wall for example – quite contrary to Wheeler’s view of a jigsaw puzzle that was largely solved – saw a sustained demolition of a chronological framework that had held the field for decades.⁴³ Similarly, basic questions were being asked about the history of Romano-British towns and villas, while diligent and especially quantitative – enquiries into artefacts such as pottery, began to introduce a new perspective into Roman economic studies throughout the Empire. We might think of the risky business of ship-wreck excavation, with its phenomenal dividends of mercantile cargoes;⁴⁴ or that more mundane, but hardly less fruitful task, of amphora-counting, with its huge potential for assessing the changing balance of trade in the Ancient World.⁴⁵ Field survey, too, came of age, as the results of John Ward-Perkins’s

pioneering project in South Etruria began gradually to be digested,⁴⁶ and compared and contrasted with the impression of agricultural history gleaned from the ancient sources. Archaeology was, all of a sudden, no longer the hand-maiden of history, but a hugely important alternative view, redolent with possibilities.

Roman art and archaeology also acquired a new respectability. Strong had put in a plea for the reintegration of Roman art and architecture with more main-stream archaeological studies, and it was not to fall on deaf ears. I spoke earlier of the entrenched view of Roman art as a phenomenon derived from and inferior to the Greek tradition. Indeed, as John Ward-Perkins once put it 'the notion of a specifically Roman art was startling and, to many, faintly ridiculous'.⁴⁷ Much work had gone on to identify the indigenous element in Roman provincial art, but the nature of the image at the core remained elusive. Thus the need for Strong's own *Roman Art*, posthumously published in 1976, a book justly claimed as a masterpiece.⁴⁸

Richard Reece had joined the Institute in 1970, as a direct result of the introduction of undergraduate teaching. With Donald Strong's sudden and lamented passing in 1973, there then came into being a new Second Triumvirate, with wide interests: epigraphy, Dalmatia, numismatics, the late Empire, the Roman army, the Arsenal Football Club and the Rolling Stones and much more. Unlike the Second Triumvirate of Antony, Lepidus and Octavian, this one of Wilkes, Hassall and Reece has had a relatively harmonious history, beyond some suitable divergence of scholarly opinion. But there are parallels between this Triumvirate and that of ancient Rome. As Sir Ronald Syme reminds us (and I quote in adapted form), in the Rome of the Triumvirs 'men became intensely conscious of history . . . History and oratory furnished suitable and indeed laudable occupation . . . [and] the retired . . . might with propriety occupy their leisure . . . in digesting the legal and religious antiquities of the Roman People'.⁴⁹

Now in Syme's account there is no mention of digging, and the Wilkes, Hassall, Reece Triumvirate does not indeed organise any Institute Roman excavations. Instead, just like their ancient Roman counterparts, they send out trained and perceptive exploratory colonists, to spread the word and report on their mission in undergraduate, graduate and doctoral dissertations. Most have settled only temporarily but, even so, here are the disciples of the new Roman archaeology who in the 1970s, and now in the '80s are implementing the lessons garnered here in lecture and seminar, in library and laboratory. I find it particularly appropriate that many have chosen to work in Italy, the heart of the Roman Empire. Some Institute students were working with me at Narce, near Rome, in the late 1960s and early 1970s, but it was, I think, the great Settefinestre Roman villa excavations that most moulded events. This splendid project to disinter one of the huge slave-run villas of Italy's western seaboard, opened a new chapter in Italy's rich history of Roman archaeological studies.⁵⁰ Alongside the excited chatter about Marxism and the decline of capitalism, were serious Institute students, intently pursuing their seeds, soils and bones. While Institute conservators lifted and restored precious wall-paintings, others were about the task of sorting and classifying the pottery that tells us so much about the villa's chronology and overseas connections. Exciting and heady stuff, not least because the excavation was in turn to provide a spring-board for a mammoth field survey of the Ager Cosanus and the nearby Albegna Valley, a project that has already achieved the status of a major landmark in Italy's Romans' archaeology.⁵¹

I pick out the Settefinestre project for special attention partly because the Institute played a formative and crucial role in its success; but also because I believe that it marked something of a watershed in Roman archaeology. The quality of the work, combined with the distinction of the results, established beyond doubt that historians ignore the archaeological evidence at their peril.

However, the Triumvirate, so very ably aided and abetted by Judy Medrington, and now by Margaret Roxan, have encouraged their disciples to journey much further afield than central Italy. I am sure that I have drawn up no very comprehensive list, nor do I intend to present you with a long catalogue of projects and their directors: but it was not very difficult to think of significant contributions made by Institute students in France, Spain, Italy, Algeria, Tunisia, Libya, Turkey, Jordan, Greece, Cyprus, Malta, Bulgaria and, of course, Britain. Moreover, let it be stressed that, as in antiquity, these enterprising agents themselves come from many countries, united by questing minds, exemplary technical skills and the deep desire to discover and understand the past. They are both of international origin and have international perspectives, academic diplomats of the modern age. They do this Institute huge credit and unmistakably reflect the wise guidance and clear leadership of the indefatigable Professor, John Wilkes. He is a magnificent scholar in the best Institute tradition and works unceasingly for the advancement both of his students and for the subject. We all, students and colleagues alike, owe him our gratitude.

But what of the future? I think that we have only to look at the remarkable fruits of great current projects like the investigation of ancient Carthage to appreciate the shape of future enquiry. Here, scientist, technician, historian and classicist can, and must, work side by side to uncover and interpret the surviving vestiges of this great city of antiquity.⁵² But again, what of the future? I have before me a recent editorial in *The Independent*, entitled 'Impoverished policy towards Universities'.⁵³ It makes gloomy reading. It begins: 'the University of Aberdeen, nearly 500 years old, is threatened with closure in 1990. Queen's University, Belfast, has stopped buying books for its library. Even Oxford University is considering cuts in all subject departments'. There follows a parade of melancholy statistics, not the least of which is that, since 1980, all but seven universities have lost at least 10 per cent of their grant. The editorial concludes by observing that, while average living standards have risen substantially in recent years, especially when measured in terms of cars, videos, central heating, washing machines and so on, 'we shall be poorer by far if, at the end of the century, we have fewer universities'.

Neither the shocking statistics nor the viewpoint are unfamiliar and I am reminded of the demise of the archaeological department at Lancaster, which I helped to found in the early 1970s; and of the increasing number of accomplished archaeologists who have left the profession. But I do not believe in a counsel of despair. The resources that we deploy are not minuscule, the number of archaeologists in post is vast compared with a generation ago and the range and variety of the projects presently underway is prodigious. Here, those who founded and steered institutions like the Institute deserve our respect and gratitude. Perhaps today we should be like one L. Munius, who was a trader from the central Italian town of Rieti. In a delightful dedication to Hercules, he offers one tenth of his profits, provided that the god helped him to get his accounts right!⁵⁴ Certainly we need all the help that we can get.

Even so, with or without divine intervention, I believe that Roman archaeology, particularly as it has been nurtured here in the Institute, has a rosy and fascinating future. I find it encouraging that no fewer than 181,000 people came to see our *Archaeology in Britain* exhibition, just recently ended; and, working for that exhibition, I found it an absorbing task to see how the approaches to Roman archaeology have changed over the last fifty years.⁵⁵ Now that you have asked me, as an outsider, to peer through a window into the fifty-year history and workings of your Institute, I have discovered where many of these developments have been wrought. As an external examiner, I award you first-class honours. Congratulations upon your Jubilee!

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The Archaeologists of Roman Britain

by R. F. J. JONES

Roman Britain has always been a small academic field, where most of the main figures have been personally well-known to each other. This means that it is not always easy to disentangle who influenced whom, and who was friendly with whom. The enclosed nature of the study of Roman Britain is reinforced by the rarity of contributions from specialists from other periods or countries. When they have come they have often been very stimulating (e.g. Hodder, 1974; Biró, 1975; Bogaers, 1979; Sommer, 1984). The reluctance of foreign scholars to comment on Roman Britain contrasts with the British tradition of being prepared to set off to enter uncharted archaeological territory anywhere in the world. This hangover of Imperial confidence can cause some bemusement among local archaeologists and among those of other countries who do not share the same foreign ambitions. The lack of interchange of ideas within Roman provincial archaeology is remarkable, when it would seem that there was so much in common. Only in the study of Frontiers has there been much common ground established; yet even the Limeskongresses are dominated by the particular and the descriptive, rather than broader analytical discussions. This is partly a function of the particularist nature of much archaeology, where so many problems can seem specific to the material handled. Yet what this has meant is that archaeology is made up of much more strongly national traditions of research than is common in other subjects, especially the sciences.

This attempt to understand those people who have contributed to the study of Roman Britain and to identify something of what has influenced their views fit a current mood of self-examination in archaeology, trying to see where the proliferation of theoretical approaches spawned in the 1970s have led us (cf. Bintliff, 1986; Hodder, 1984). Within this discussion the idea that different people at different times create different images of the past has gained popularity. To anyone acquainted with historiography this may not seem much of a revelation, but it apparently has been so to many archaeologists who have cherished notions that the material nature of our evidence somehow conveys a special objectivity to our interpretations. How spurious such notions are can be clearly demonstrated in an extreme case by the interpretations put on the archaeological data from the Southern African Iron Age, determined not so much by any kind of objective assessment of that evidence, but by the political and intellectual contexts of modern South Africa (Hall, 1984). It is therefore reasonable to examine who have been the students of Roman Britain in order to understand what kind of study we have created. This paper is mainly based on published works, enhanced by accumulated informal information. A full-scale study involving interviewing surviving people would be instructive, but a rather larger enterprise than this can be.

At first the archaeology of Roman Britain seems to offer a smooth passage. Here no choppy water need be expected from debates on theoretical orientation:

I have based my work on no specific philosophy of history or ideology, other than the belief that what men and women have done in the past is worth pursuing and remembering for itself (Salway, 1981: vii–viii).

There is widespread support for the idea that not being aware of your theoretical base is the same as not having one. Salway's approach belongs firmly in the tradition of Romano-British studies, aiming for a descriptive narrative of unique events. That tradition has shown itself to be remarkably resilient, despite a certain lack of confidence that the subject of Roman Britain was valid at all. Several authors have felt the need to preface their works with almost an apology for writing on Roman Britain (Haverfield, 1915: 9; Collingwood, 1932: 1–3; Salway, 1981: 3). In discussing Salway's book, Barry Cunliffe pointed out how little research aims in Roman Britain have changed in the last half century, while 'Romanists (with a few notable exceptions) have plodded along the straight and narrow, reluctant even to cast enquiring eyes on the activities of kindred disciplines' (Cunliffe, 1984: 177). The reaction to Cunliffe's remarks shows a complacent belief in traditional approaches which has survived tenaciously among students of Roman Britain, but not uniquely there. European historical archaeology as a whole has been slow to feel the influence of new ideas in other branches of archaeology. In Britain there have been slow stirrings in medieval archaeology (Rahtz, 1981; Hedges, 1982), but such work remains in the minority. In America the 1980s have seen the opening of a comparable debate between 'New Archaeologists' and Classical Archaeologists (Renfrew, 1980; Dyson, 1981; Renfrew, 1983; Wiseman, 1983; Snodgrass, 1985; Dyson, 1985). The strong tradition of particularism and description rather than analysis throughout protohistorical and historical archaeology in Europe means that it has been virtually impossible to identify groups following different theoretical stances (cf. Kuper, 1973, for British anthropology). Until recently, on this everyone simply agreed.

Part of the reason for this came from the lack of confidence in the material evidence traditionally felt by classical archaeologists in the face of written sources. It is therefore crucial to examine the relationship between written and material sources. Can protohistoric and historic archaeology be accepted as a mature contributor to our understanding of the past, or is it forever condemned to be no more than a provider of illustrations for more serious books? Many historians still subscribe to the latter view. It might be expected of those whose main interests lie in the narrative history of events, or of the detailed internal politics of a régime: it is more surprising among social and economic historians working with a wider perspective (e.g. Finley, 1971). Similarly, despite abundant experience to the contrary (cf. Gillam, 1974), there are archaeologists who still believe that archaeology in the field can solve cruxes in the written historical record. Part of the problem seems to lie in failures to understand developments in both aspects of the study of the past. Modern archaeology both in practice and in theory has made great strides towards providing well-founded statements about topics such as settlement patterns and exchange networks. Meanwhile in history the growth of longer perspectives of study should be very appropriate for archaeological collaboration. The work of the *Annales*

school of historians, especially Fernand Braudel, should be to the taste of archaeologists, for its concern with long term processes and the relationships between human activity and such aspects as the natural environment, climate, and technology. The conflict between extreme propagandists of documents and material remains is more apparent than real, as both are attacking shadows of their opponents which are no longer there. Strong statements may have been necessary to make the point that archaeology as the newcomer needed to be taken seriously. Yet the more mature view is that both types of source have their contributions to make, provided that both are treated with appropriate rigour and valued for the kind of evidence that they can properly yield (cf. Reece, 1984; Reece, 1987; Driscoll, 1984; Rahtz, 1984). To a considerable extent documents and archaeological data are different in character, yielding different kinds of information, so that they can rarely be directly tested against each other (cf. Randsborg, 1985: 447; Hodges, 1983: 25). We should not be surprised when the different sources present us with contrasting perspectives. The diversity of view should be a strength of understanding protohistorical and historical periods rather than a weakness.

Three big books: drawing the blueprints

There have been many books written on Roman Britain through this century: nearly all have accepted the historical framework. Only three have been widely accepted as the authoritative statements for their generations. The first was Francis Haverfield's lecture to the British Academy in 1905, *The Romanisation of Roman Britain* published in expanded form in 1912 and modified in subsequent editions from 1915. In nine chapters he dealt with the romanisation of the Empire; preliminary remarks on Roman Britain; romanisation of Britain in language; romanisation in material civilisation; romanisation in art; romanisation in town-life; local government and land-tenure; romanisation in religion; chronology of the romanisation, and the sequel: the Celtic revival in the later Empire. The illustrations included plans of town-houses and villas, and coarse pottery and brooches. In a slim volume, Haverfield effectively set the agenda for Romano-British studies for more than sixty years. The book is laced with references to the rest of the Roman Empire and frequently turns to the classical literature. It was heavily dependent on the written sources, but did make use of the recently discovered evidence from such excavations as Silchester. Haverfield's analytical approach set a style that was followed later by Collingwood (1932) and Rivet (1958) and strikes a sympathetic note with the modern reader.

A quarter of a century later R. G. Collingwood wrote the next major work, which took a primarily narrative form (Collingwood and Myres, 1937). His contribution to the *Oxford History of England* was a much more ambitious book than Haverfield's, making the first authoritative synthesis of the history of Roman Britain. Its influence has been enormous, expressing the views derived from the rapid expansion of field research in the 1920s and 1930s, forming the framework of argument through the 1950s and 1960s, and still being regularly quoted in the 1980s. Collingwood was also a distinguished philosopher and probably the most substantial intellectual among all Roman Britain scholars. His book was thus much more than an attempt at a simple narrative. Collingwood made full use of archaeological data, but

retained a predominantly narrative form. The book was a thorough and convincing analysis of all the major issues, drawing conclusions that were controversial. They have been followed, opposed and now seem to be becoming fashionable again, for example on the parasitical relationship between town and country in Roman Britain. Yet it is sobering to realise how small was the body of reliable archaeological evidence available to Collingwood, compared to what we have now accumulated over the subsequent half century. A difference of style between Haverfield and Collingwood was acutely recognised by Richmond (1957: 3–4), arguing that Collingwood's intellectual approach provoked inquiry whereas Haverfield was himself so authoritative that he almost silenced it. That their works are still valued reading is both a compliment to their vision and a criticism of our more recent reluctance to tamper with the agenda of study which they created.

The only serious modern challenger to Collingwood's book has been Sheppard Frere's *Britannia*, published first in 1967 and revised in 1974 (Frere, 1974). The period since Collingwood wrote produced a further transformation in the archaeological material available, which Frere integrated into a coherent contribution to what remained explicitly in its sub-title 'a history of Roman Britain'. Although it lacked some of the flair and challenge of Collingwood's book, *Britannia* had remained a staunch guide and support in the last two decades, during the most rapid expansion of all in information on Roman Britain. Yet there is now a discrepancy between the approach of what is still the standard work on Roman Britain and the way archaeological data are currently being used in other areas of archaeology, and increasingly in Roman Britain itself.

All three of these books are heavily underpinned by major contemporary discoveries through excavation. Each author reacted quickly to new results. This was most marked in their treatment of towns: Haverfield drew on the early campaigns at Silchester; Collingwood on Wheeler's Verulamium excavations, Frere on his own work at the same site. Each set of excavations indirectly formulated a generation's views on Romano-British urbanisation. To understand the influence of the three great peaks in Romano-British studies, it is necessary to look beyond them to other achievements and to the people who have created them.

The 1920s and 1930s: building a framework

Understanding the connections within Romano-British archaeology becomes somewhat easier with the growth of the subject after the First World War. From then it is possible to identify important networks in the development of the subject, some based on universities, some on excavation projects. Haverfield died in 1919, but as Richmond implied he does not seem to have stimulated an active group of followers: many had been killed during the war. The 1920s and 1930s were dominated by a powerful set of scholars whose shadows linger still. Their work established the essential framework for modern research on Roman Britain, defining the artefactual record and the chronological sequence. The senior figures were F. G. Simpson and Mortimer Wheeler, as well as R. G. Collingwood. Both Simpson and Wheeler had begun their careers before 1914 and both came to exert most influence through their excavations. There the similarities ended. Their personalities seem to have been utterly different: Simpson quiet

and modest to the point of finding difficulty even in writing his excavation reports. Wheeler the aggressive publicist, pushing his ideas on excavation and archaeology through promoting what we would now call his image. Wheeler's work in the 1920s at Brecon, Caernarvon and Caerleon rapidly laid out the framework for understanding Roman Wales; it was followed up in less spectacular ways by V. E. Nash-Williams. When Wheeler moved on to the London Museum and then to found the Institute whose jubilee is here celebrated, his excavations at Verulamium and Maiden Castle reached a much wider academic and general public. However, although Wheeler was clearly in contact with his peers such as Collingwood and Richmond, not many important figures emerged in the field of Roman Britain as a direct result of his influence at this period. Perhaps the Second World War caused too much of a disturbance. Certainly after the war Wheeler himself became increasingly concerned with archaeology of a much wider geographical span. The most prominent of his protégés before the war, John Ward-Perkins and Kathleen Kenyon, later earned their highest reputations for work far beyond Roman Britain. Perhaps also Wheeler's personality was so powerful that he influenced most effectively people who remained at a distance, rather than his closest associates.

Simpson had been conducting important excavations in the north of England for several years, particularly on Hadrian's Wall. However the crucial season for establishing a new scheme for understanding the northern frontier came at Birdoswald in 1929. There he was joined by two young archaeologists, Ian Richmond and Eric Birley, then respectively 27 and 23. Richmond and Birley took responsibility for the publication of the excavation and the elaboration of the new interpretation of Hadrian's Wall (Richmond and Birley, 1930; Birley, 1930). As they progressed to great eminence in Roman studies in later years, it is easy to forget how young Richmond and Birley were at the time of these discoveries. The four period scheme for Hadrian's Wall became the basis for the many subsequent campaigns carried out by the group that built up around Richmond and Birley, Simpson and Collingwood. The scheme provided the opportunity to impose a new discipline on the northern archaeological material, particularly the pottery. Pioneer work was done on the pottery made in Roman Britain in the 1930s by Philip Corder and M. R. Hull in Yorkshire (cf. Evans, 1987), but the great classification of Romano-British coarse pottery came from John Gillam in 1957 (Gillam, 1957). This provided a quite unparalleled precision in defining a major class of everyday artefact and opened up new possibilities for the study of the masses of pottery found in excavations; the interpretation drew directly on the four period scheme for Hadrian's Wall.

After the Second World War: refining and expanding the record

Richmond and Birley both went on to distinguished service as university teachers, Richmond at Newcastle then Oxford, Birley at Newcastle then Durham. From their university bases it is easier to follow the growth of groups of like-minded scholars, both students and colleagues. Birley at Durham led the most clearly defined school, including John Mann, Brian Dobson, Michael Jarrett, John Wilkes, Roy Davies and David Breeze. What characterised the group was an abiding concern with the organisation of the Roman army, for which the most useful

evidence naturally came from epigraphy and other written sources. Over the years, the group came to show less enthusiasm for archaeology in the field; some members did little more than dabble with excavation, in contrast to the activist period of the 1930s. John Gillam at Newcastle carried much of the post-war burden of excavating in the Hadrian's Wall region. Richmond himself did less there. At Oxford Richmond supervised a set of students with a rather different perspective. People like Barri Jones, Malcolm Todd, Charles Daniels and Richard Reece all showed a commitment to field work in one form or another and, more distinctively, a concern with the wider world of the archaeology of the Roman Empire. At roughly the same time in the 1950s another centre was growing up less formally around Sheppard Frere's excavations at Verulamium. In various capacities, John Wacher, Brian Hartley, Bill Manning, Mark Hassall, and John Peter Wild were connected with the project. Frere's Verulamium work set a new tone. The aim was to seek much more refined detail, rather than the bold strokes of Wheeler in the 1930s. There was also some shift of interest towards social and economic issues.

Many of the young men from all three of the networks identified were able to find jobs in universities in the late 1950s and the 1960s, as universities expanded. However archaeology departments were rare and several of them were employed in history or classics groups. The coverage of the country was, however, impressive: Jarrett and Manning at Cardiff, Jones and Wild at Manchester, Hassall and Reece at London, Wilkes at Birmingham, Todd at Nottingham, Daniels at Newcastle, Wacher at Leicester, Hartley at Leeds. This generation later found themselves well placed to achieve professorships, mostly in the rapidly expanding 1970s: Wilkes at London, Todd at Exeter, Jarrett, Manning, Jones and Wacher all at their original universities. However, although by the 1970s most of those who had been in history or classics had come to rest in archaeology departments, very few of this group had themselves been able to study archaeology as undergraduates. Some had come into the subject by unlikely routes, but most shared a training in classics or ancient history. It is hardly surprising that a framework derived from the written sources has retained a strong hold on the way of writing of many of this group.

This group was transitional in that its members came to maturity as the British Empire came to an end. The events of the outside world gave new uncertainties and new opportunities for fresh ideas at this time. There had long been a tendency, especially in earlier writers, to identify a beneficent Roman Empire with a similarly beneficent British. Many British scholars looked from the perspective of the coloniser, bringing civilisation to grateful natives. For the archaeologists emerging in the late 1950s and 1960s such an approach became increasingly unsatisfactory as the old confidence of the British Empire slipped away. Yet even in 1987 it remains hard to shed all the intellectual complexes generated by our own Imperial past. If it now seems easier to persuade ourselves that we can assess imperialism more objectively, it is still hard to understand. A tension must still be recognised between dominating and civilising. In post-Imperial Britain it is increasingly popular to take a stance overtly sympathetic to the perceived victim of Roman imperialism, the native Romano-Briton. The problem is perhaps best exemplified by whether Roman Britain is regarded as an episode in the British past or as an outpost of the wider Roman Empire. Clearly it was both, but it is sometimes hard to

combine the two views. Perhaps we should all come clean and make explicit from what viewpoint we write.

The transitional generation taught in the Archaeology first degrees newly established or revitalised at many universities in the late 1960s and early 1970s. Those of us who have been students since then and some of our teachers have tended to a different approach to Roman archaeology, drawn from a wider acquaintance with other periods and specialisms in archaeology and from changes in the subject itself, which has embraced the results of new techniques of scientific analysis, palaeoecology and computing. It has become much harder to see the Roman period as automatically special, just because we are dealing with the material remnants of people who might have read Cicero. We ourselves were able to read books that were the fruits of the late 1950s and 1960s and which looked at Roman Britain much more in social and economic terms, such as *Town and Country in Roman Britain*, *The Civitas Capitals of Roman Britain*, and *The Roman Villa in Britain* (Rivet, 1958; Wacher, 1966; Rivet, 1969). Much of the credit for this trend belongs to Leo Rivet. Although he was working in some isolation at Keele University in a classics department and did not create any obviously influential school of pupils, his published work stimulated the thinking of many younger scholars. Now a significant number of those who were students in the first half of the 1970s have been able to find posts teaching Roman archaeology in universities or in influential posts in archaeological units. Networks can be detected around several foci. To declare my own interest, I am part of two such groups, from Manchester and from the London Institute itself. From Manchester in the early 1970s came Bill Hanson (Glasgow University), John Lloyd and David Kennedy (Sheffield University), Nick Higham (Manchester University), Mick Jones (Trust for Lincolnshire Archaeology), and Paul Bennett (Canterbury Archaeological Trust). Studying in London at about the same time as me were Tom Blagg (University of Kent), Martin Millett (Durham University), Simon Keay (Southampton University), Tony King (King Alfred's College) and Andrew Poulter (Nottingham University). Regular visitors to the Institute at the same time were Mike Fulford, Martin Henig, Ian Hodder and Keith Hopkins.

The way this new generation has come to occupy positions which will lead to their being influential has fixed the pattern for the rest of this century, at least in the universities. Field archaeology seems a little more flexible. However, although the professional field archaeologist has an increasingly important part to play in advancing the subject by actually recovering the primary evidence, the nature of funding for public archaeology in Britain has not encouraged field archaeologists to develop their work beyond the basic study of individual rescue projects. Despite distinguished contributions by many from outside the university system, such as David Breeze or David Miles, Stephen Johnson or Richard Hingley, it seems likely that the responsibility for the overall interpretation of Roman Britain will lie with those in universities. The paradox is that in the present structure of British archaeology they will not be responsible for the majority of the field research on Roman Britain. Recent and current restrictions on universities have meant that the 1970s group will grow in influence in the subject, if only through the passage of time. It will also find its views largely uncontested by younger challengers beginning their academic careers, as very few will be newly recruited to universities until the generation of the 1950s/1960s are due to retire around the end of the

century. The immediate future of the study of Roman Britain is thus in the hands of the 1970s group of archaeologically trained specialists. Do we have a consistent view now? How will we go forward into the next century, when we will have turned into the senior figures in the subject?

The present: archaeological emphases

Cunliffe (1984, 178) saw 'Britannia as an aged, cossetted old lady, sitting immobile in an airless room reeking of stale scent, fawned on by a bevy of tireless, dedicated servants'. He based this lurid image on publications from the late 1970s and early 1980s. The gloom he felt can be lifted by some books which have tried to extend ideas on the Roman period in one way or another. Works such as Charles Thomas's *Christianity in Roman Britain* (1981), David Peacock's *Pottery in the Roman World* (1982), and Keven Greene's *The Archaeology of the Roman Economy* (1986) have all discussed the archaeological evidence in fresh ways to draw more or less controversial conclusions, rather than simply describing the material. However, the most positive advances have come not so much from single-author volumes but from a series of conference proceedings, mostly published by British Archaeological Reports. In *Studies in the Romano-British Villa* (Todd, 1978), *Invasion and Response* (Burnham and Johnson, 1979), *The Roman West in the Third Century* (King and Henig, 1981), *The Romano-British Countryside* (Miles, 1982), and *Military and Civilian in Roman Britain* (Blagg and King, 1984) are to be found a range of papers which attempt to analyse the evidence rather than just to describe it. The papers in these volumes can be supplemented by others in journals like *World Archaeology*, the *Oxford Journal of Archaeology*, and the *Scottish Archaeological Review*. Such attempts to address issues of cultural processes are informed to varying degrees by ideas drawn from anthropology, economics, geography and elsewhere. Most, but not all, have been written by members of the younger generation of Roman Britain specialists. There is then something of a consensus emerging that the interesting questions in Roman Britain in the 1980s are those like the interaction of native and external influences and the nature of the systems collapse at the end of Roman Britain. Such questions are to be investigated not only by changing the frame of reference by including ideas from adjacent areas of study, but also practically by looking at diet and agriculture through faunal and floral evidence (King, 1984; Jones, 1982; van der Veen, 1987) or by using scientific analyses to identify exchange patterns (Evans, 1986). Similar approaches are to be found in the Netherlands (e.g. Brandt and Slofstra, 1983), but are still poorly established in other areas of Roman provincial archaeology.

Yet the tradition of descriptive writing remains strong, especially in the most prestigious journal for Roman Britain, *Britannia*. As a new journal with its first volume in 1970, it might be expected that *Britannia* would have reflected the new ideas that have been developed during its lifetime. The truth is the opposite. *Britannia* has been seen as a 'journal of record', presenting evidence rather than ideas. This has been demonstrated in the types of paper published:

Category of paper	1970-9	1980-6	Totals
Site-based studies:			
(i) excavation reports	31	30	61
(ii) monuments/field work	14	14	28
Small objects/typology, including pottery	28	22	50
Art and architecture	9	7	16
Papers based on written sources (topography, textual and historical commentary)	24	16	40
Epigraphy/prosopography	14	2	16
Frontiers/army organisation	13	8	21
Numismatics	11	1	12
Synthesis/cultural process	11	2	13
Fauna and flora	3	1	4
Other	3	0	3
Totals	161	103	264

The categories chosen and the allocation to categories are admittedly subjective. Also, some papers that are mainly about sites or classes of object do contain discussions of wider issues. However the general trends are shown clearly enough. About one third of all papers published have been site-based, mostly excavation reports; they have taken up rather more than one third of the space because of their greater length than most articles, sometimes as much as 60% of the pages of the main articles in a volume. The tone of the other papers has generally also been descriptive rather than interpretative. The differences are also striking between the patterns in the first ten volumes under Sheppard Frere's editorship and the subsequent six under John Wilkes and then Malcolm Todd. There is a clear rise in the proportion of site-based papers from 28% of all papers in 1970-9 to 43% in 1980-6. This has happened at the expense of the variety of topics covered: in the 1980s hardly any epigraphy or numismatics, or synthesis.

A similar exercise to this has recently been published by Stephen Dyson, comparing the development over fifty years of *American Antiquity* and the *American Journal of Archaeology* (Dyson, 1985). From very similar patterns at the beginning, those two journals have diverged in character from about the 1960s. Whilst the kinds of topic appearing in *AJA* have remained remarkably static over half a century, *American Antiquity* has come to offer 'a balanced presentation of all subdisciplines and approaches to archaeology' (Dyson, 1985: 454). This has included a substantial representation of papers on cultural process and fauna and flora. It is alarming to think that the young journal *Britannia* is following more closely the pattern of the stilted recent history of *AJA*, rather than the more open and inclusive style of *American Antiquity*. The reasons for this trend do not simply lie in lack of intellectual adventure. To some extent, *Britannia* has always relied on papers being offered to it, and is therefore dependent on what people choose to send to it. Much of the work which has appeared in the various conference volumes was specifically commissioned, with the result that the authors published there material which otherwise might have been offered to *Britannia*. What is more, most excavation reports published have been subsidised by the bodies that paid for the

excavation. Clearly such papers fill a volume very economically, even if it has led to some reports appearing in *Britannia* which might have been better suited to a regional or county journal. Yet the quality of individual papers is not at issue: their standard has always been high. It is the balance of topics covered which causes concern. It cannot be said that all is healthy with the study of Roman Britain when the main national journal is not the main forum for discussion of the main issues of interpretation.

The future: interpreting the archaeology

It would be wrong then to think that in the study of Roman Britain the initiative has now firmly passed to those who see it as a part of archaeology as a whole and wish to analyse it as such. Many remain as insulated from ideas in other areas of archaeology as other archaeologists are from Roman Britain. Yet the passage of time is likely to strengthen the influence of those committed to more archaeological and analytical approaches. Through the sheer quantity and quality of our data, present and future archaeologists of Roman Britain ought to be well-placed to take a lead in developing new ideas for archaeology as a whole. There is a growing realisation that protohistoric Europe holds such a wealth of archaeological evidence, abundant in quantity, well-classified, closely dated, and supported by some written records, that it provides a real testing ground for the value of archaeology. If meaningful statements about the past cannot be produced here, what hope is there for making sense of a few projectile points from New Mexico? Concepts like systems collapse and centre-periphery relations are obvious tools for trying to understand Roman Britain (cf. Bloemers, 1987, on the Rhineland). Yet they can only be applied if work is conceived at a regional level and within a broad chronological bracket.

At the level of theory we must be prepared to keep close contacts with adjacent disciplines which also are concerned with the same kinds of issue, while still recognising the distinctive nature of archaeology and the peculiar complexities of Roman Britain. In practical terms, the first task must be to evaluate thoroughly the evidence we already have. An unthinking rush into excavation or laboratory is unlikely to yield much of the real new value. Although I cannot believe that we already have enough data to answer any question we might wish to ask, the simple repetition of traditional forms of data collection is likely to lead to returns of diminished use. On the other hand, selective practical work in the field and laboratory is likely to be profitable where based on a full critical assessment of what we do and do not know. One of the greatest obstacles to advances in the future is likely to be the organisational separation of the scholars who are needed to share ideas and co-operate in practical projects. We have to find ways to collaborate effectively between individuals in public and university archaeology, as colleagues in the Netherlands have succeeded in doing over the last decade (cf. Brandt and Slofstra, 1983). The increasing complexity of our evidence requires replacing the lone individual by the team of specialists complementing each other. Yet team work in the field and laboratory can only be a part of the future. Satisfying syntheses will still need to be produced from individual minds. The signs are hopeful, as the beginnings of these attitudes can be found in Romano-British archaeologists of all generations. If we are

to make the most of the efforts that have gone into all the excavation and study of the period, a new sense of intellectual excitement has to be fostered to attract new minds to the topic. They will give fresh impetuses in their own directions, overturning the cosy assumptions in today's new ideas.

Finally, the vast majority of Romano-British archaeologists in this century have had one thing in common: we have been men. There have been many women interested and knowledgeable in Roman Britain, but most have written as specialists on some class of objects or another and have not written works of synthesis. This is odd. It is not so true in other periods of archaeology, nor in other countries, let alone in other disciplines. It takes us back to Cunliffe's image of Britannia as an old lady with her bevy of dedicated servants, nearly all of them male! If we can open some windows to let some fresh air in for Britannia, perhaps more women will get in too.

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A Question of Time? Aspects of the Future of Pottery Studies

by MARTIN MILLETT

Introduction

There are few periods after the Neolithic for which pottery does not comprise a substantial part of the surviving material evidence. For this reason alone pottery has always held a prominent place in archaeology, whatever the aims or methods of its study. Nonetheless there is a surprising absence of literature concerned with the methods used for its study and their theoretical foundations. Some reports on pottery from a variety of contexts provide insights into these aspects, but archaeologists have generally failed to stand back from the data to contemplate it and evolve new approaches which might provide novel insights. This paper presents one, personal, view of some directions in which developments might lie, evolved from work principally on Romano-British pottery (Millett, 1979; 1983). It is, however, the contention that they have a broader relevance which should enable more information to be derived from one of the main types of material that archaeologists have at their disposal.

Chronological problems

Up to now, pottery has largely been used for dating. Before we can understand and use such dating further we need more fully to appreciate its nature. In virtually all contexts, pottery is dated by comparative means; by comparing the group of material under examination with others for which dates have already been established, whether through association with other dated material or by its historical context. The date given is then based on that of the most similar group. Provided that *groups* rather than *individual sherds* are used this seems a reasonable method.

A refinement of the technique is to seriate the groups on the assumption that the types within the groups behave in a regular way through time rising steeply to a peak in frequency before declining more gradually (Fig. 1). This too seems reasonable and various methods have been successfully implemented, but it should be noted that the shape of the frequency curve is a matter of assumption since few stratified sequences have been critically examined to provide empirical validation of the shapes of their curves. It seems a basic essential for future work that closely dated sequences are examined to provide this information and establish the parameters within which they vary.

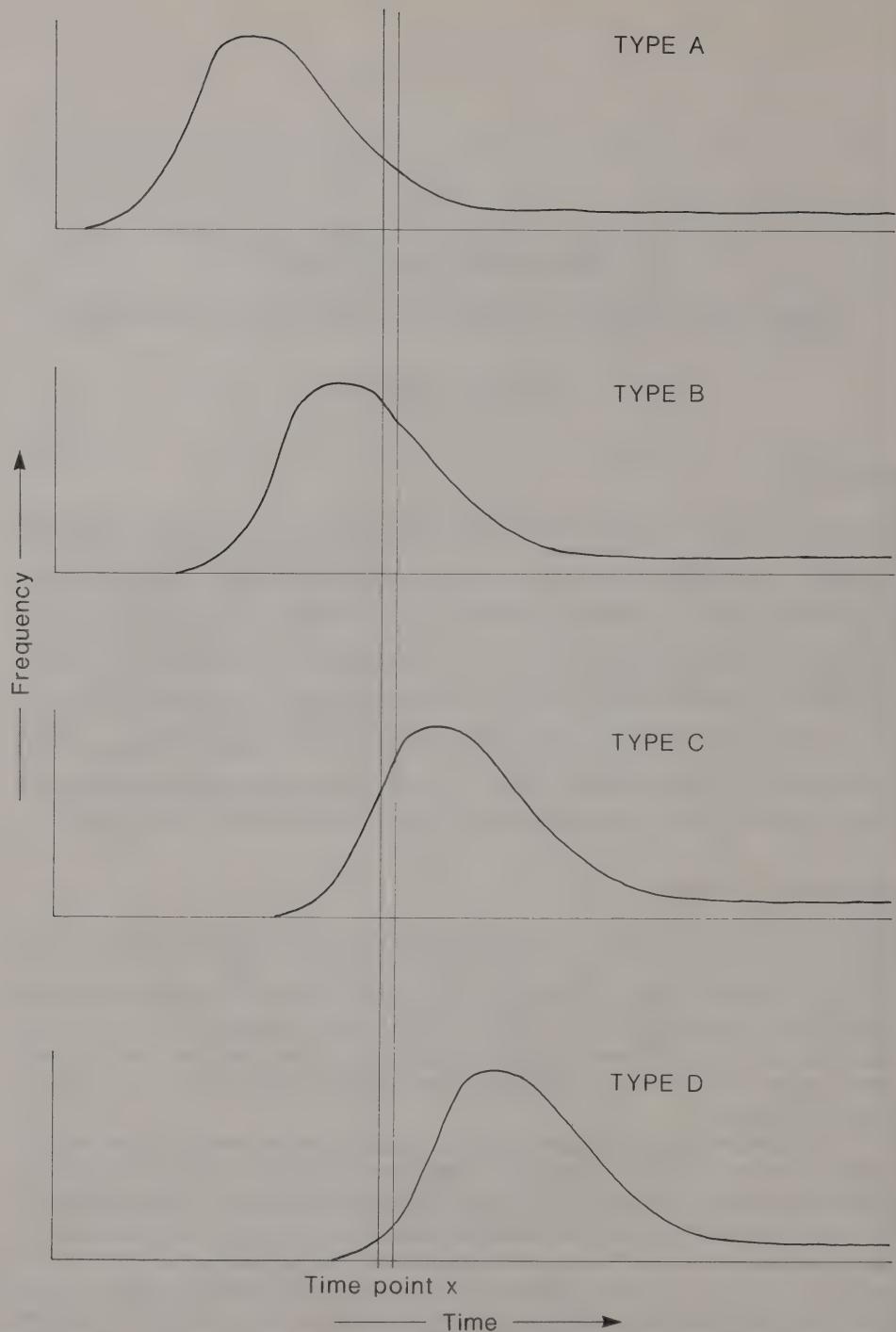


Fig. 1 Hypothetical frequencies of a series of pottery types introduced at four different stages and their representation in a sequence of *rubbish deposits*, illustrating their 'normal' representation at one particular time point, X.

Whether comparative dating is founded on an intuitive approach, or one based on a reduction of the data to a numerical form, it is vital to recognise that the archaeological date derived is an *aggregate date of deposition*, based on the behaviour of the type in a series of dated contexts. Thus with a sequence of types introduced through time, their representation in a deposit at time point X will normally be a result of where that time point lies in relation not to their date of production, but to their *aggregate 'lifespan'*. This results from most of the types with which they are being compared having been discarded as rubbish after use, often when the production of the type had ceased (Fig. 2). This has not always been recognised by archaeologists who have sometimes developed sophisticated procedures for estimating the average age of sherds at deposition (Orton, 1978) without taking into full account the fact that the date already by definition incorporates this element.

The problem arises when dealing with deposits which are not 'normal' in that they comprise material deposited 'prematurely', usually as a result of accidental destruction, before they would normally have arrived in a rubbish deposit (Schiffer, 1976, 33 refers to these as *de facto* deposits). In these instances the proportions of the types represented at time point X will be larger than 'normal' for the newer types and the assemblage will even contain some which are so precocious as to make the deposit appear much later than it is (compare Fig. 1 with Fig. 3). This effect has been clearly demonstrated in the British sites destroyed by Boudicca in AD 60 (Millett, forthcoming).

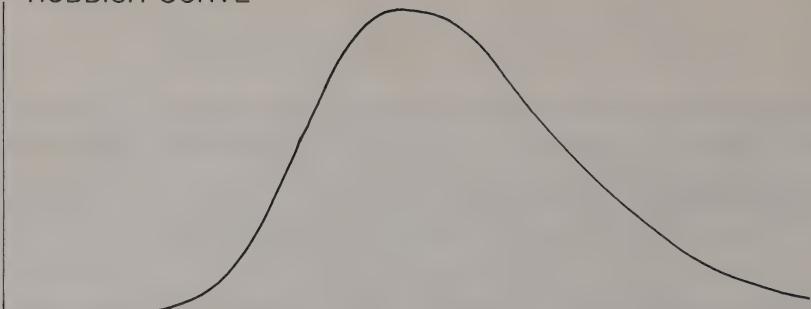
This aspect of the nature of pottery dating has two major repercussions which are central to all its archaeological uses. First, the nature of the deposits used to produce dating sequences is vital if reliable dates are to be established. Not only is a full understanding of residuality essential (Evans and Millett, forthcoming), but so also is the routine identification of Shiffer's *de facto* deposits which may well bias dating seriously. Given the difficulties one would normally recommend that rubbish deposits are to be used for the construction of ceramic sequences. This means that excavators should always be attempting to explain the formation processes leading to the accumulation of deposits on their sites.

Secondly, one must seriously question the use of the *terminus post quem* (*TPQ*) in dating based on pottery. This is simply because, unlike a coin for instance, where the date used is that of production, the date for a dated potsherd is an *aggregate date of deposition of the other known examples* and can only ever be presented as a date range. The date of the context within which it is found is therefore either the same as or later than that *range*. Where, as should be the normal practice, assemblages of pottery are used for dating purposes it should be possible to establish a firm range for the date of deposition. Any application of the *TPQ* will cause the date given to be systematically too late, especially if the common, but erroneous, practice of using the end point of the range is followed. Again, a careful understanding of both the nature of the dating and of the context is essential, but the use of the *TPQ* is generally inappropriate.

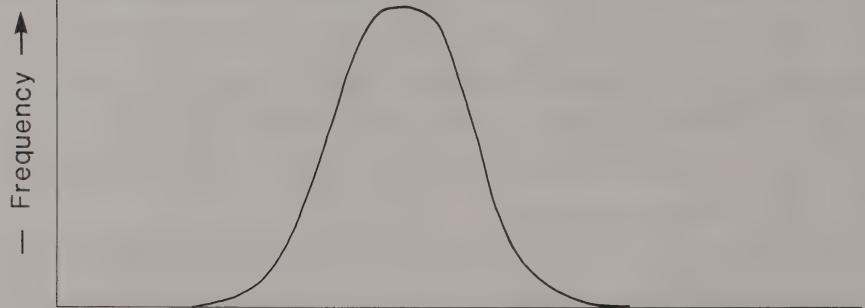
Chorological prospects

Most past studies of excavated pottery have been used to establish relative or absolute chronologies. Behind this are a series of implicit assumptions concerning the relative

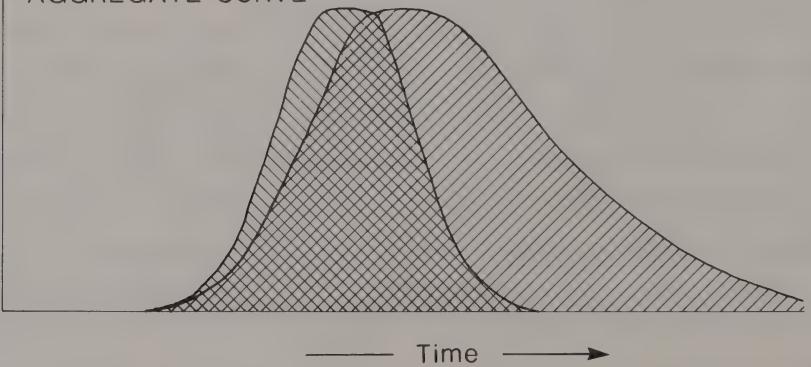
A RUBBISH CURVE



B PRODUCTION CURVE



C AGGREGATE CURVE



— Time —→



Production only



Production and
rubbish deposition



Rubbish deposition only

Fig. 2 Hypothetical frequency curves for pottery types through time distinguishing the curve normally used to derive a pottery date (A) from that of production (B).

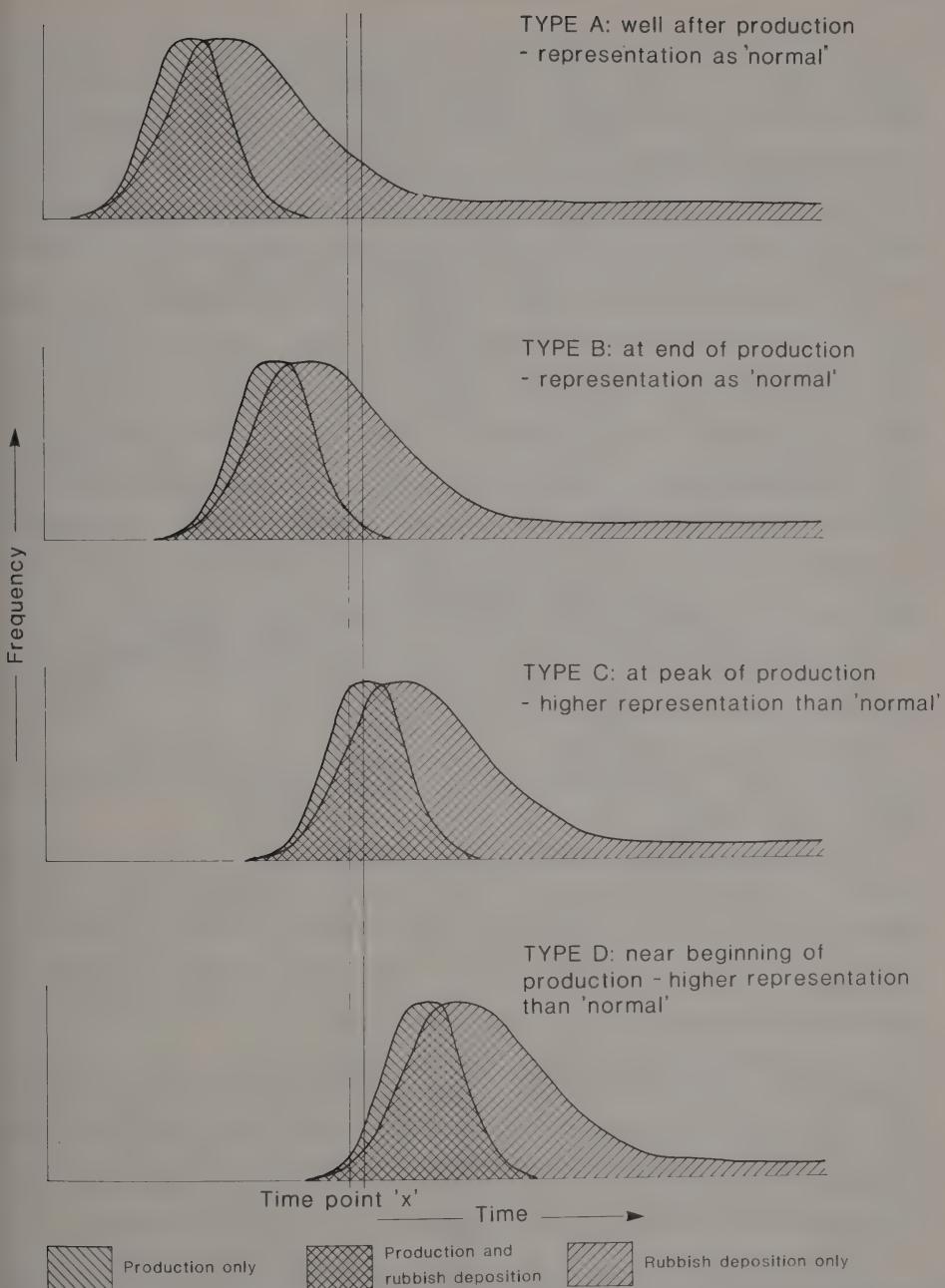


Fig. 3 Hypothetical frequencies of a series of pottery types introduced at four different stages and their representation in a sequence of deposits, illustrating their representation in a *destruction deposit* at one particular time point, X.

importance of the determinants of variability between assemblages. Central to these is the idea that change through time is the most important factor. Thus in the establishment of ceramic chronologies it is assumed that at any point in space groups of pottery deposited close together in time will be similar, whilst those further apart will be more dissimilar. This basic assumption underlies all the uses of pottery for dating but suffers from two flaws. First, it has rarely been tested so, although undeniably reasonable, there is little empirical evidence to support it or to calibrate it so that we can know how similar groups have to be for contemporaneity or non-contemporaneity to be assumed (Millett, forthcoming, note 10).

Secondly, considerable work has now been done which demonstrates that what Childe (1956, 15) called *chorological variation* has a very real impact by causing discernible dissimilarities between pottery assemblages. These variations are those which cause differences between groups within the same temporal zone. Thus the substantial work that has been achieved in the analysis of pottery to establish exchange patterns, symbolic distinctions and ethnic differences between social groups or functional and social/economic differentials within them show that the basic assumption upon which pottery dating has been based needs to be carefully assessed. This is simply because dissimilarity can be caused by archaeologically interesting factors other than time, so these need to be isolated and their influence assessed before they can be taken into account in the interpretation of pottery. This is not a negative point, for the potential archaeological value of pottery is enhanced by its recognition.

Figure 4 illustrates the types of factor which have been shown to be influential in causing measurable variation between assemblages. These may be grouped into four dimensions; three chronological, one chorological. These four dimensions are:

- A. The **primary time dimension** – change resulting from normal alterations in fashion, trade, technology etc, in the society concerned. This is the normal chronological change fundamental to all archaeological study.
- B. The **residual time dimension** – influences on the composition of the assemblage which are diachronic. The most obvious of these are the influences of residual material in the assemblage resulting from the deposition of material from an earlier period in a later group. The processes concerned are such that the length and nature of the previous history of the site and the character of the deposits from which the pottery is derived are important variables which need to be carefully considered (Evans and Millett, forthcoming).
- C. The **imposed time dimension** – factors which are imposed by the subsequent site history, including the archaeologist's input. Thus the subsequent occupation history of the site will influence the quality of preservation and the degree of disturbance of the groups of finds, thus the size of the assemblages and composition (in sherd size for instance). Similarly the quality of the methods of archaeological recovery, together with the perceptions and methods of those classifying the material for study, will have a major impact on the nature of the data.
- D. The **chorological dimension** – the factors which determine variation between groups in the same time zone. There are clearly a number of such factors, the following of which have been shown to have a measurable impact on assemblage composition:

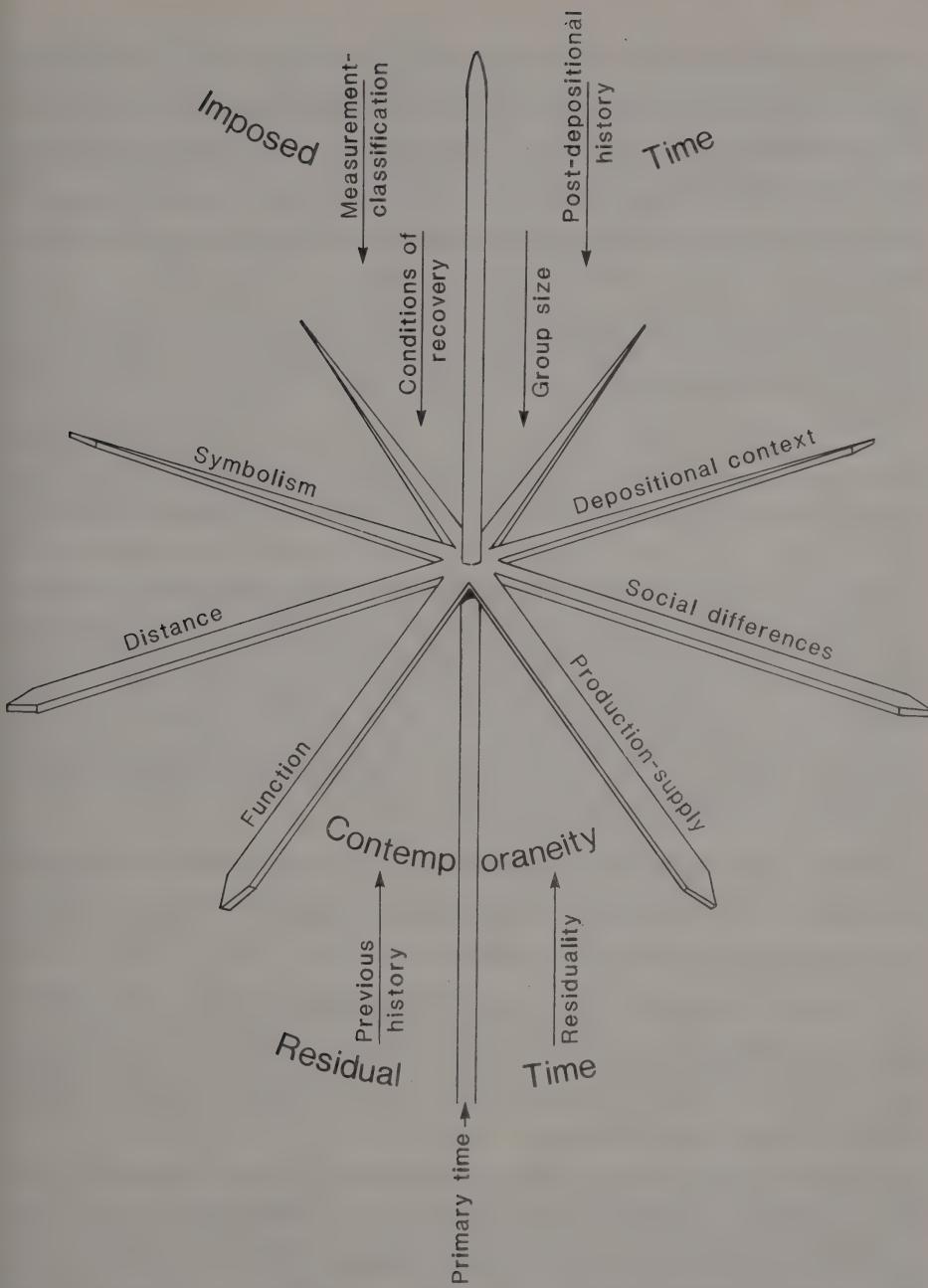


Fig. 4 Conceptual model of the relationship between the identifiable variables which influence the composition of a particular pottery assemblage.

(i) *Production and supply.* Factors which vary with the social context, available technology and economic conditions and also have a strong spatial component, such that assemblages deposited at the same time with differential access to exchange systems may be very different in composition. An obvious example of this are the differences between groups from the forts and contemporaneous farmsteads in the region of Hadrian's Wall in the 2nd century AD. The former had access to a Romanised exchange network and received large quantities of factory made pottery brought from a wide geographical area. The latter rarely have more than a handful of sherds and show a much more limited range of types and sources.

(ii) *Social differentiation.* This factor clearly has influence in some instances, although it has rarely been explicitly recognised. Such differences have been identified in the assemblages examined from Boudiccan contexts in Roman Britain (Millett, 1983) and have been shown to be influential in post-medieval urban contexts at Duisburg in West Germany, for instance, where the pottery from individual buildings can be related to the social status of the occupants through the documentary sources (Gaimster, pers. comm.). It seems highly probable that similar patterning will be identified elsewhere when the pottery is examined in relation to other data.

(iii) *Function variation.* Differences in site function have been discerned through the nature of the ceramic assemblages in several contexts (e.g. Millett, 1979). The most encouraging results have been achieved when the patterning discovered can be independently related to spatial patterning, for instance through architectural evidence. Such a correlation of pottery assemblages has been noted in material from the Late Geometric site at Zagora on Andros (Cambitoglou *et al.*, 1971; Millett, 1983, 214–23). There the pottery groupings produced by a multivariate analysis of the types present showed a very closely similar pattern to the architectural grouping of the rooms from which they were derived.

(iv) *Distance.* The simple distance decay function has a demonstrable effect on group composition because of its effect on the intensity of interaction between groups (Hodder, 1978). It should however be noted that this effect may often be produced by the aggregate effect of factors (i) and (ii) above and (v) below.

(v) *Symbolic uses.* Hodder (1982) has stressed the variation which can occur between assemblages because of the use made of material culture to symbolise and signify group differences. Such differentiation has also been identified in parts of later Roman Britain where areas, like that of the Parisi, exhibit ceramic assemblages which are distinct from those of neighbouring areas (Evans, 1985).

(vi) *Depositional context.* Schiffer (1976) and Carver (1979) have both stressed the importance of the nature of the process leading to the creation of the archaeological record. These factors are also influential in the composition of the finds assemblage with, for instance, the sherd size and group homogeneity being closely related to the type of rubbish disposal pattern.

Given that these factors have already been isolated and others may well be identified, it

is clear that our methodology should be improved to take their effect into account when examining our data. The direction in which such progress can be made is difficult to assess.

If the similarity between pottery assemblages is perceived of in the same way as that between individual artefacts in numerically based classification (Doran and Hodson, 1975, 190-7) then it may be expressed generally as the number of attributes in common in relation to the number that describe the groups being compared. This produces a neutral measure of similarity which can be generated and manipulated using standard computing packages (like Clustan and SPSS) and which is close in its nature to the concepts used intuitively by archaeologists. Nevertheless it suffers from the problem that there are no definable distinctions between the similarities caused by the various chronological or chorological factors. As a result we need a methodology to relate similarity or dissimilarity to cause. As the similarities are created by a multiplicity of factors which are interrelated we cannot expect simple uncausal relationships.

The approach adopted in my research and which may have potential for further development in contexts where mass produced and standardised pottery was used, is a multi-stage procedure which relates the pottery to its archaeological context and to its position within the contemporary cultural repertoire. First the pottery needs to be designed to a series of cross-cutting classifications which should be explicitly designed to reflect what are *a priori* likely to be causes of variation (Hodson, 1980). Thus, for instance, an assemblage might be divided into series of types which are functional (and likely to relate to types of usage), stylistic (likely to relate to changing fashions and time), and also fabric (likely to relate to geographical origins and exchange). When the material has been so classified, assemblages should be compared on the basis of each classification using techniques of multi-variate analysis to generate similarity matrices which can be scanned and processed by a Principal Components Analysis. Examination of these results should isolate the main dimensions of variation which can be related to external site evidence to establish their meaning. So, for instance, stratigraphic data may be used to identify variations reflecting chronology. Provided that the typologies used for the different analyses were well founded, the patterning identified should relate to the causes of variation postulated in their establishment. Thus, for instance, the stylistic typology should be strongly related to time. The identification of other causes of variation may be less easy to interpret unless independent evidence for calibration is available.

Whatever the difficulties, such methodology at least enables an assessment of the complexity of the data to be made and avoids the assumption that time is always the main factor. A secondary advantage is that a scrutiny of the correlation matrices generated during the analyses also provides useful information about the relationships between pottery types by indicating which are most usually found together. A comparison of the results of the different analyses produced using different classifications can also provide valuable insights into the nature of the site. This is particularly important in reintegrating pottery studies with the study of the site itself. In this way we should look forward to pottery no longer being relegated to an appendix in an excavation report but becoming central to the integrated interpretation of the societies which generated the archaeological record. In doing this we should see that ceramic change is not simply a question of time.

Acknowledgements

This paper is derived from parts of my D.Phil. thesis which was written under the supervision of Professor S. S. Frere to whom I am most grateful. I should also like to thank Jeremy Evans, Colin Haselgrave and Richard Reece who have commented on the ideas incorporated. David Gaimster kindly allowed me to refer to his data.

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Scientific Investigation or Cultural Imperialism: British Archaeology in the Third World

by WARWICK BRAY and IAN C. GLOVER

Introduction

This paper has been developed out of the lectures delivered by the two authors on 17 February 1987. The texts presented then were written independently at a time when one of us was in South East Asia and the other in London. Later, when discussing how we might integrate our various experiences and perceptions, we felt that, despite the great differences in the cultural surroundings of our field research, we had been led to rather similar ideas about the role (moral, philosophical and practical) of British archaeologists working in Third World countries. However, for publication we felt that the two contributions should remain distinct, since each complements the other. They follow, in the order they were presented, with some amendments to the oral texts.

PART 1: British archaeology in South East Asia and Australia: science or imperialism? (I.C.G.)

Should the research of British archaeologists working abroad be viewed as the legitimate activity of an objective and universally valid scientific method of understanding the past? Or is it no more than a form of cultural imperialism, part of the intellectual apparatus whereby predatory western capitalist states manipulate and control other people's knowledge of their own past (Gore and Root, 1986)? This might seem to be an extreme position to take, but it is an argument being put forward in cultural situations as different as aboriginal Australia, Black Africa, Hispanic America, and by some Amerindian groups in the United States (Hammil, 1987).

My own concern with these problems comes from several sources, from my involvement in prehistoric research in South East Asia over the past twenty years and, particularly, by an appreciation of the very different value given to, and levels of, archaeological research in the countries there – Indonesia, Philippines, Vietnam, Thailand, Malaysia and Burma.

The experience of editing in 1981–82, jointly with Professor Bruce Trigger, two volumes of the journal *World Archaeology* on the development of regional and national traditions in archaeology strengthened those impressions. One particular issue which we were concerned

with at that time (Trigger and Glover, 1981) was whether the diversity of approaches to, and styles of archaeology throughout the world were a reflection of the immaturity of our discipline or would always be present. Another source of inspiration has been the stimulating recent volume *The Past is a Foreign Country* by Professor David Lowenthal (1985) and the various papers in the Symposium *Archaeological 'Objectivity' in Interpretation* held at the World Archaeological Congress in Southampton last year.

Indonesia and South East Asia

In 1980 at a conference in Bern devoted to the cultural links between Europe and Indonesia I argued that archaeology, and especially prehistoric archaeology, was an alien European concept and practice introduced into Indonesia in the days of Dutch colonial hegemony and refurbished in a period of European and American intellectual dominance in the mid-twentieth century. I argued that Indonesian prehistory as reflected, for example, in the books of H. R. van Heekeren (1957, 1958, 1972) or in the most recent regional prehistory by Peter Bellwood (1985), was an abstract mental construct, satisfying certain concerns of Western bourgeois society, but of no significance to more than a handful of Indonesians. Following from this was the implied conclusion that research into prehistory by the few Indonesians undertaking it was likely to be a derivative and sterile occupation with no roots in Indonesian culture, satisfying none of the desires many Indonesians have to know more about the past of their own societies. This rather pessimistic appreciation of the state of prehistoric archaeology in Indonesia has not been overthrown by what I have read, or seen of research there, over the past few years (Glover, 1986). In trying to account for this, two factors seemed significant.

First, the whole concept of prehistory was a European one and I had to consider, briefly, the development of prehistory as an intellectual and methodological discipline in the context of social, political, and commercial links between Europe and the peoples of Asia, Africa and the Americas. Professor Bruce Trigger's (1984) distinction between what he calls 'nationalist, colonialist and imperialist' archaeologies provides a useful framework for this exercise. Second, the pioneers of Indonesian prehistory were the Swiss naturalists and ethnographers Fritz and Paul Sarasin, followed by a number of Dutch scholars: especially P. V. van Stein Callenfels, A. N. J. van der Hoop, and H. R. van Heekeren. The intellectual environment of early twentieth-century continental Europe profoundly influenced the direction of these early studies. Even today a very large part of the investigations into Indonesia's past have been carried out by European researchers. The latest and most authoritative interpretation of Indo-Malaysian prehistory has been presented by Peter Bellwood (1985), a British-born, Cambridge-trained prehistorian now working from Australia. Nothing comparable has yet been produced within Indonesia. This intellectual and material contribution of Europe, creating, in effect, an Indonesian prehistory, was the main theme of that earlier lecture.

The first generation of prehistorians working in Indonesia, Bühler, van Stein Callenfels, van der Hoop and van Heekeren, or academic researchers such as R. Heine Geldern, all adopted, to a greater or lesser extent, the dominant mode of archaeological procedure of the early twentieth century: concentration on material form and typology for the recognition of culture groups and culture areas, and explanation of all change in the archaeological record as

the result of the diffusion of techniques or the migration of peoples from one culture area to another. This may have satisfied contemporary European perceptions of the structuring of social processes, but it meant little to the people of Indonesia. The paradigm, almost universally held in the heyday of European colonial rule in Asia and Africa, that societies do not change without external stimulation, played a part, if a minor one, in giving intellectual support to colonialism. It denigrated indigenous cultures, characterised them as uninventive and static, and put them on a level with 'primitive' phases of European development, thus helping to justify the 'civilising mission' of Europe in bringing backward native peoples up to the cultural level of the twentieth century.

Trigger (1984: 360–63) has characterised 'Colonialist Archaeology' as a distinct mode of archaeological thought and shown how it was variously applied in the United States, Africa, and Australasia. I quote from his discussion on the situation in African archaeology.

'During the colonial period, archaeologists and ethnologists regarded the so-called tribal cultures of sub-Saharan Africa as a living museum of the past . . . The most spectacular example . . . is provided by the controversies surrounding the Zimbabwe ruins. Early white investigators of these monuments . . . saw them as proof of ancient white settlement in southern Africa, by Sabaeans or Phoenicians. Cecil Rhodes appreciated the propaganda value of such speculations. When in 1904 the archaeologist D. Randall-MacIver dated these ruins to the second millennium AD he so angered local whites that it was almost twenty-five years before serious archaeological work was again carried out there. Although Gertrude Caton-Thompson confirmed Randall-MacIver's work, and the Bantu origins of Zimbabwe in 1930, amateur archaeologists kept alive the notion that Zimbabwe was the work of foreign invaders, merchants, or metalworkers. For white settlers such claims served to deprecate African talents and past accomplishments and to justify their own control of the country. Extraordinarily, in 1971, Peter Garlake was forced to resign as a Rhodesian Inspector of Monuments because he was unwilling to interpret Zimbabwe to the satisfaction of the white settler government of the day.'

In the 1980s nationalism has replaced colonialism as the prevailing spirit in Africa bringing a bizarre new twist to the interpretation of Zimbabwe.

Recent prehistoric research in Indonesia

After a long hiatus systematic research into Indonesian prehistory by foreign archaeologists started again with my own research in East (then Portuguese) Timor in 1966–67, the Australian-Indonesian Expedition to Sulawesi in 1969, G. J. Bartstra's reinvestigation of the Pacitan techno-complex in Java from 1972, and Bellwood's work in Minhasa, Sangihe, and Talaud in 1974. Research by Indonesian archaeologists had been kept going through R. P. Soejono's surveys and investigation of stone cist burials in Bali, but it was not until the excavations at Gilimanuk in West Bali from 1963 (Soejono, 1979) that research by Indonesians into their own prehistory gained some independent momentum.

Competing archaeologies – imperialist and colonialist

The work, of the foreign archaeologists at least, was now undertaken with a rather different

research mentality. In place of the colonialist view that all change comes through diffusion or migration from outside, a neo-evolutionary adaptive model was generally followed, in which all societies were seen as dynamic and changing over time, seeking to manipulate natural environmental systems in such a way as to maximise the rate of energy flow into culturally preferred directions. A sort of uniformitarianism of social processes was accepted, but the forms and structures of society were seen as being transformed over time by the interaction between accumulating knowledge and the forms of organisation and technology specific to particular places and times.

Trigger (1984: 363–64) characterises this new approach as part of the ‘Imperialist Archaeology mode’, particularly as influenced by the New Archaeology of the 1960s. By stressing internal change and adaptation it eliminated previous tendencies of colonialist archaeology which stigmatised native peoples by failing to recognise their independent creativity. Yet the new approach was interested in the archaeology of various parts of the world simply as data with which to establish generalisations about human behaviour. It denied the intrinsic value of national traditions or local cultures in much the same way as colonialist and evolutionary archaeology of the late nineteenth and early twentieth centuries saw a value in the ‘primitive’ cultures of Australia, Africa and the New World only to the extent that they helped Europeans to understand their own past.

Nationalist archaeology

In some newly-independent countries, and where native peoples appear to be being denied the right to their cultural identity, forms of nationalist archaeology have developed in reaction to the ‘colonialist’ and ‘imperialist’ modes.

Trigger (1984: 358–60) believes that prehistoric archaeology has almost everywhere arisen out of an attempt to satisfy nationalist aspirations. Without accepting that this is the only inspirational source, it is clear that the development of European prehistory was much stimulated by the post-Napoleonic surge of nationalism and Romanticism. This is particularly well exemplified in the case of Germany where the humiliation of defeat and occupation by France led to a glorification of supposed ancient Teutonic military values whose physical remains were eagerly sought and displayed in the cause of a revived German Empire. In Ireland, Israel, Mexico, Iran before Khomeini, Vietnam and the Libya of Gaddafi among many other places, it is easy to see how archaeology has served, if not been created by, a growth of national consciousness. Trigger argues that nationalist archaeology is strongest among peoples who feel threatened, insecure, or deprived of their political rights by more powerful nations; or in countries where appeals for national unity are being made to counteract serious divisions along class, or ethnic lines. Nationalist archaeology tends to emphasise the more recent past and to draw attention to the political and cultural achievements of past civilisations with visible, monumental architecture and centralised political structures. Earlier prehistory, or the archaeology of small-scale pre-literate communities, tends to be ignored by nationalist archaeology. Nationalist archaeology, not surprisingly, tends to replace colonialist or imperialist archaeology in newly independent countries. The popularity of classical and Islamic archaeology in Indonesia at the expense of prehistory can be seen as an example of this

principle. In order to see what nationalist archaeology is capable of today we need to return to Africa.

Earlier I mentioned that Garlake had been forced to resign his government position on account of his refusal to interpret the ruins of Great Zimbabwe in a way acceptable to the white settler government of Rhodesia. In independent Zimbabwe, we find that an attempt is now being made to appropriate the monuments for the purposes of Greater Shona nationalism (Garlake, 1984: 123). It has been claimed by Dr Ken Mufuka (1983), the first African Director of Museums and Monuments, that not only does Great Zimbabwe belong to the indigenous African past, but that only black Africans can properly understand and interpret the monument.

The assertion that only 'native peoples' can interpret their own past – that it is their exclusive property – is finding expression also in North America and Australia, once the happy playground of imperialist archaeology; a sort of 'palaeolithic preserved', a continent-wide experimental laboratory where hypotheses relevant to European stone age archaeology, or law-like generalisations about culture process, could be tested by British-trained graduates making their academic reputations and on their way to well-paid jobs. Not so long ago Murray and White (1981) characterised Australian archaeology as 'Cambridge in the Bush'. Recently, white Australian archaeologists have been forced to compromise with aboriginal interests. I quote from a representation by R. F. Langford (1983) on behalf of the Tasmanian Aboriginal Community:

'You say that as scientists you have a right to study and obtain information of our culture. You say that because you are Australians you have a right to study and explore our heritage, because it is a heritage to be shared by all Australians, white and black . . . we say that you come as invaders, you have tried to destroy our culture, you have built your fortunes on the lands and bones of our people and now, having said 'sorry', want a share in picking over the bodies of what you say is a dead past. We say that it is *our* past, *our* culture and heritage, and forms part of *our* present life. As such it is *ours* to control and it is *ours* to share on *our* terms' (Langford, 1983: 2).

Clearly the academic discipline of prehistory, exported from Europe, and successfully grafted on to white Australian society, has been (at least for the moment) decisively rejected by the people whose past archaeologists seek to study. It has been rejected on several grounds:

- 1 That European notions of history and man's biological and cultural evolution as enshrined in archaeological practice are quite different from Aboriginal ones.
- 2 That archaeological work does not serve Aboriginal interests, which are to gain control over land, and thus their own destiny.
- 3 That Aborigines are passionately interested in the specific details of their own past and resent having their heritage serve as an experimental ground where white academics can test general theories and advance their own careers.

Cultural imperialist archaeology then, as did colonialist, arose from the intellectual preoccupations and political needs of Western society at a particular stage in its own evolution. It

has, I argue, no more relevance or value for the people of the Americas, Australia, Africa, or Indonesia, than did the paradigms of earlier generations. I would argue that European contributions to the prehistory of Indonesia and Australia, though many in quantity, have not been very significant. Although we have constructed some sort of prehistory for the region it is for external consumption only and of little relevance to interests in their own past of the peoples living there.

Vietnam and Thailand

I have emphasised research in Indonesia and Australia because I have been involved in some of this work and the literature is reasonably accessible, but I also want to say something about the rather different situations in Thailand and Vietnam. In early 1986 I was able to spend two weeks in Vietnam and to discover at first hand the very considerable achievements archaeologists there have made in unravelling their own prehistory. It is a paradox that a country materially so poor, barely recovering from over thirty years of war against the Japanese, French, Americans and Chinese, has devoted so much attention to archaeology. Perhaps we should say even 2000 years of warfare, for Vietnam was colonised by Han China in the first century AD, gained its independence only in the tenth century, and suffered repeated invasions from China before falling under French domination in the mid-nineteenth century. The past has a moral force in Vietnam, unequalled perhaps anywhere in the world, and in the process of trying to reassert a Vietnamese nationality archaeology plays an important part. The Vietnamese see in their Bronze Age, what they call the Dong Dau, Go Mun and Dongson periods (from the late second to late first millennium BC) the first flowering of native Vietnamese genius, the creation of a territorial political state, or states, with high levels of technical and artistic skills, before their subjection to Han Chinese imperialism. The practice of modern archaeology was introduced to Vietnam by the French early this century (although in the newly independent Ly dynasty of the eleventh century we find already a developed antiquarian interest in the remains of the Dongson civilisation) and today it is rather a mature discipline, guided by a Marxist-Leninist philosophy, put to the service of nationalism. Scholarly debate and inquiry, however, seem not to be stifled by this political framework and I was heartened to find that Vietnamese archaeologists disagree with each other just as much as we do in the West.

In Thailand, by contrast, archaeology was, until about twenty-five years ago, confined to art-historical studies of sculpture, temples, painting and fine arts; it was mainly the prerogative of aristocratic, western-educated Thais on the fringes of the royal clan. Structurally, archaeology is still taught and studied within the University and Government Department of Fine Arts. Starting in the early 1960s the systematic investigation of Thai prehistory was initiated by a series of joint Thai-Danish, British, American and New Zealand expeditions and, until about 1980, most sustained work in Thai prehistory was carried out by overseas researchers, being published (if at all) in English. The late start for prehistoric archaeology in the country can be explained largely by the fact that alone of South East Asian countries, Thailand was never the colony of a Western power. Thais took their past for granted, historical traditions were not well developed and the country was an agrarian, quasi-feudal state with

limited literary traditions. A centralised political and administrative system developed only from the middle of the nineteenth century. Over the past twenty years, however, Thailand has been undergoing dramatic social and economic transformations; it is now much more urbanised, secondary and tertiary education has been widely extended and, following the political upheavals in neighbouring countries and some within Thailand itself, a new generation of Thais has emerged which require new explanations of their past. With few genuinely historical sources, Thais are looking with enthusiasm to archaeology to provide them with this new understanding, but of course the questions they seek to answer from archaeology are not necessarily those favoured by overseas archaeologists. In particular they show rather little interest in generalising explanations couched in terms of evolutionary processes, not very much in archaeology outside of Thailand, and if there is one thing which they are seeking to achieve at the moment it is documentation of the wealth and variety of the material remains of man in Thailand over the past four or five thousand years. As in Indonesia, but in marked contrast to what is happening in Vietnam, most resources devoted to archaeology in Thailand go into the reconstruction of the great temples and ancient cities of the Khmer, Sukhothai and Ayutthaya phases of the Thai medieval period. This is archaeology in the service of both nationalism and business, for Thailand is a country where tourism is a major industry.

As in Indonesia, I believe that overseas archaeologists working in Thailand are out of step with the concerns of Thai scholars investigating their own past. While there are ethnocentric, anti-foreign tendencies within the Thai academic community, they do not at the moment have wide support. Thailand is a society tolerant of diversity and Thai archaeologists still welcome the presence of foreigners working there – even if they are not too interested in their conclusions – for they see them as a source of new techniques and analytical procedures to stimulate indigenous Thai archaeology (Charoenwongsa 1985).

Synthesis

Earlier in this paper I referred to the question of whether we could ever expect to see the emergence of a single coherent discipline of archaeology which will be applied to the material remains of man throughout the world and which would develop the same sort of data and interpretations whether it was undertaken by say, Eskimo archaeologists in Africa, or Vietnamese in Ecuador. Trigger's (1986) conclusions were that whereas the technical procedures of archaeology will be increasingly standardised, the problems people seek to resolve from the study of archaeological remains arise out of the historical experiences of each community and each generation, and that we must not expect our discipline to develop in the same way as have the natural sciences, geology or mathematics over the past 300 years.

Finally I want to return to the main problem addressed in this paper. Does the work of British archaeologists overseas, especially in Third World countries, serve any useful purpose for the host society: is it a morally neutral activity of interest to us, and harmless to them, or is it, as I caricatured it earlier, a form of cultural imperialism and exploitation which appropriates other people's pasts and serves to strengthen the economic and cultural domination of western capitalist states? Recently Professor Lowenthal (1985) showed that all our reconstructions of the past are, in a sense, fictional and that we travel into the past only to give some

meaning to our present. If we accept this does it imply that archaeological and historical 'science' has no legitimacy, that there is no objective reality or permanent value to our reconstructions of states and process in the past? Yet, as Trigger (1986: 15) points out, the data of archaeology are not entirely a construction of our own mind, even if their recording and analyses are coloured by our specific concerns with the past. All societies need this 'foreign country' which is their past and which is better entered through the exercise of historically and archaeologically trained imagination, than by subverting and appropriating it for narrow nationalist or racial purposes, as in Nazi Germany in the 1930s. If the discipline of archaeology, as practised by British archaeologists in Third World countries helps to give to us, and perhaps also some individuals from those countries in which we work, a 'passport' to enter, examine and respect their pasts, then I will accept that while much of our effort serves the suspect aims of cultural imperialism, the exercise can still be justified.

PART 2: British Archaeology and the Third World: A Latin American Perspective (W.B.)

In a way, the dual organisation of this paper reflects the traditional insular British view of archaeology: that Europe, the Classical cultures and the great Near Eastern civilisations come first and the rest of the world, the 'other bits', a poor second.

At one time it was a custom in this Institute to begin a lecture by summarising what Gordon Childe had said, then to decide whether he had got things right or wrong. It is significant, then, that he said almost nothing in public about the archaeology of Africa, the Far East, Australasia or the Americas and, from all accounts, what he said in private was not very complimentary. In this ethnocentric view, the mainstream of human history (i.e. of *our* history, Parr, this vol., pp. 30–31) flowed from the Fertile Crescent, through Classical lands, to the Europe of Bronze Age aristocrats and Celtic chieftains. Now, two Directors later, the Institute has made amends and we have specialists in Africa, Asia and Latin America on our staff. As the balance of the Institute's teaching has changed, so too has the political and intellectual world in which people like us have to function. Former colonies, and the Third World countries in general, are no longer willing to be mere receivers of ideas and foreign specialists, but have begun to develop indigenous archaeologies of their own.

This is something we have to come to terms with, as a country that has traditionally exported its values and methodologies (as well as its archaeologists) to faraway places where there was little local competition. This was strictly a one-way traffic. I can think of no more than six foreign directors of excavations in the United Kingdom. All of them came from Europe or North America and their influence on the course of British archaeology was entirely good. By contrast, the number of British archaeologists who have worked overseas is impossible to calculate; if we divide the world into archaeological donors and receivers, Britain stands out as a prime example of a donor country.

As Ian Glover has shown in the first part of our contribution, a generation ago there was only *one* archaeology. Its theoretical basis derived ultimately from the European tradition (counting North Americans as honorary Europeans) and, consciously or not, it was designed to satisfy the needs of Western scholarship. With independence, this archaeology has lost its

unique authority. Scholars in the emergent nations are, quite rightly, beginning to ask: What kind of archaeology do we want? What are our intellectual and political priorities? As part of the same phenomenon, we in Britain must reconsider the nature of our own relationship with the countries in which so many of us work. What has happened in practice is that there have been many individual arrangements, with each segment of the profession fighting for its own interests, but there has been no overall, national policy, nor much awareness that one may be needed.

British archaeologists in Latin America

To illustrate the haphazard way in which things have generally come about, we need only look at the history of British archaeological participation in Latin America, picking up the story in the early nineteenth century, just after the wars of independence.

The Mexican prehistorian José Luis Lorenzo (1981) has described those days in an article published in *World Archaeology*:

'There appeared on the scene at this time certain personages, a group made up of consuls and businessmen, on the one hand, and cultured adventurers, on the other. The diplomats had little to do; besides which they were occasionally unable to establish official relations because several changes in government might have taken place between the date of their appointment and that of their arrival at their duty posts . . . The adventurers were open to anything and ready to go anywhere. Romanticism was in full flower and not only the British were influenced by the early Victorian mode. It was a time of fascination with the exotic and of quest for stirring experiences.'

'The countries of Hispano America were criss-crossed from end to end and from coasts to mountain tops by persons who took pride and pleasure in bearing up under all kinds of discomfort and even in risking their lives . . . They represent the ideological shift that was taking place towards positivism, science and progress. Their interest and devotion had a purpose; to show the world that America had had civilisations comparable in splendour to those of Egypt, Greece and Rome.'

Many of these travellers were Europeans or North Americans and the list includes several Britons. Their accounts, often published in best-selling books, provided the first accurate descriptions of New World cultures for the general public in this country and the artefacts they sent back formed the nuclei of some of our most important collections.

The roster of 'cultured travellers' is long and diverse, from the nitrate shippers and Royal Navy officers who sent back tomb groups from the desert coasts of Peru and Chile, to the entrepreneur and showman William Bullock, who displayed casts of Aztec sculpture in his Exhibition Hall in Piccadilly (Alexander, 1985). This building, in the Egyptian style, was not demolished until 1905, and was the scene of many famous events, including the day when Bullock blocked the horse-drawn traffic of Piccadilly with the carcase of a whale on its way to dissection. Frederick Zeuner and Ian Cornwall, in the formative years of the Institute's bone collection, would have recognised a fellow spirit. Among other notables, Edward Whymper, the mountaineer who conquered the Matterhorn, brought back archaeological, geological and

entomological specimens from the Ecuadorian Andes (Whymper, 1892); Thomas Hutchinson, British Consul in the port of Callao and friend of Mary Kingsley from his days in Fernando Po, sent items from Peru (Hutchinson, 1873); and the geographer William Bollaert (1860) wrote a fine book on New Granada, Ecuador, Peru and Chile, a work which I still consult regularly.

British activity in Latin America was, in fact, more intense during the nineteenth century than at any time in the twentieth; it produced a steady flow of papers to the Royal Geographical Society, the British Association, the Ethnological Society, the Anthropological Institute, and even to county societies. At the centre of this web, receiving artefacts for his own collection, giving his opinions, and making known the work of others, was General Pitt Rivers (Chapman, 1984). Sadly, this initial impetus was not maintained into the period of professional archaeology and, with the exception of some activity in the Maya zone and of the pioneering excavations by Geoffrey Bushnell (1951) in coastal Ecuador, British involvement fizzled out.

The British and the Maya

The one area of America where some sort of continuity can be observed is the Maya region; Elizabeth Carmichael (1973) gives an excellent summary of the careers of twelve British Mayanists, the first of them (Viscount Kingsborough) born in 1795, and the last of them alive and active today.

The early researchers were concerned mainly with recording rather than excavation. Kingsborough himself paid for the copying of all the extant pictorial manuscripts and bankrupted himself trying to publish them, dying at the age of 42 from jail fever contracted in a debtors' prison. The next on the scene, Colonel John (or Juan) Galindo, visited the ruins of Copan and drew some of the carved monuments, and he too died prematurely, killed at the age of 37 in one of the many battles between the newly independent republics of Central America.

After this inauspicious start the list of archaeological draftsmen continues with John Herbert Caddy at Palenque, Frederick Catherwood (artist companion of John Lloyd Stephens on his travels all over the Maya zone) and, in more recent times, Alfred Percival Maudslay, who made seven trips to the Maya between 1881 and 1894. Maudslay was one of the great pioneer figures in the study of Maya epigraphy. He published the first scientifically accurate copies of Maya inscriptions (the basis for subsequent decipherments) and, in the most difficult conditions, made a fine series of paper and plaster casts which can be seen in London and Cambridge today.

Even at that time, women were contributing to archaeology in Latin America. One of these was a formidable lady traveller, Miss Adela Breton, who journeyed on horseback all over Mexico, ending up in Yucatan where, with Maudslay's encouragement, she made coloured copies of the wall paintings at Chichen Itza and the stucco facade at Acanceh. Her collections, sketchbooks and drawings were donated to Bristol City Museum and, with the deterioration of the original monuments, are now a prime source of evidence for art historians.

The first real excavations were carried out by the slightly eccentric Thomas Gann, a

medical officer in British Honduras. Part of his collection went to the Museum of the American Indian in New York and the rest is divided between museums in London and Liverpool. For a while, from 1919 to 1938, Gann was Lecturer in Central American Archaeology at Liverpool University, the first person ever to hold such a position in Britain. I have yet to come across anyone who attended his lectures, if indeed he ever gave any, and he seems to have trained no students. The lectureship died with him and the subject was not taught again until 1967, when the present post in London was created.

One of the sites that Gann discovered was Lubaantun, later excavated by the British Museum in 1926–27 under the direction of Captain Thomas Athol Joyce, a man of cheerful disposition who is remembered both for his textbooks and for his habit of whistling and singing in the Museum. From him there is a direct link to Adrian Digby (who excavated in the limestone caves of British Honduras) and to Eric Thompson, who worked with both Gann and Joyce. Under the auspices of the Field Museum (Chicago) and the Carnegie Institution of Washington, Thompson went on to become the grand old man of Mayan studies, ending his days with a knighthood from Britain and with decorations from the governments of Spain, Mexico and Guatemala. Thompson died in 1975, which takes us into the period of modern field archaeology, and new excavations at Lubaantun by Norman Hammond (now of Rutgers University), with the British Museum once again among the sponsors. It is at this point that the Institute of Archaeology makes its first appearance in the story, in the person of Duncan Pring, one of our first Ph.D. students in New World archaeology. Working as ceramicist for Hammond's subsequent project at the site of Cuello, Pring identified and described a new, very early, style of pottery that added a thousand years to Maya prehistory (Pring, 1977; Hammond *et al.*, 1979). After this encouraging start, our student research has continued with studies of Maya ceramic technology and burial practices.

From this long historical digression a number of significant points emerge:

- 1 The concentration of effort in British Honduras (now Belize) has its roots in colonial history.
- 2 The thread of continuity that binds the story together is a fragile one and there were rarely more than two Americanists working in Britain at any one time.
- 3 It was the museums, not the universities, which kept the subject alive.
- 4 In consequence, nearly everyone on the list was either self-taught or had moved from some other branch of archaeology. Many of these people were unable to pursue their careers in Britain and one was forced out of archaeology altogether. Prospects for our more recent Ph.D. candidates are as gloomy as ever.

This, to put it mildly, seems neglectful of one of the world's great cultural and artistic traditions, and in the rest of Latin America things were even worse.

Britain and the Third World today

I would now like to move on from the past to the present. When we were considering the scope of this paper there was a great temptation to play safe, to stick to historical survey and to avoid politics entirely, on the grounds that any political statement is bound to offend somebody or other. In the end we decided we could not, and *should not*, avoid these issues.

The main justification is, simply, that whenever an archaeologist elects to do fieldwork in another country (Third World or not) this is a political act, sometimes even a diplomatic one – witness the uneven distribution of British Schools abroad, with six in Islamic or Near Eastern countries, one in sub-Saharan Africa, one (now defunct) in South East Asia, and none at all in the Americas. This concentration in just one area of the world simultaneously reflects Childe's view of *What Happened in History* (1942) and the British government's view of What Matters in Politics. It has also had a distorting effect on the distribution of funds, on the degree of official support the investigator can expect, and on the opportunities for making a professional career. Clearly, too, there are countries where, for reasons more to do with international politics than with scientific research, it is unfortunate that British archaeologists are currently unacceptable.

In many ways, though, this kind of political problem is the least serious. It is beyond our control (the visas and permits either come through or they do not), and the individual archaeologist does not have to search his conscience over it. What concerns me much more, since questions of personal and scientific morality are involved, is a very different kind of political issue, one that is rarely discussed in print (though much talked about in certain circles), and which is a matter of emotional attitudes rather than of cold reason.

In a Jubilee Lecture it is permissible to make a personal statement and I must begin by saying that I work in Latin America because I enjoy it; with only one exception in more than twenty years, I have been welcomed there and made to feel at home. What follows, therefore, comes from a well-wisher, not from a hostile critic.

First of all, then, I would like to remind my Third World colleagues of the dangers of stereotyping. In statistical terms, every British-born archaeologist under the age of 50 or so (in other words, most of us) has spent his entire professional life in a post-Colonial world. We have oppressed no-one and we are not responsible for whatever sins an earlier generation may have committed. It is now time for all parties to put aside outdated attitudes and to consider how best to organise ourselves in the world of today.

Ignoring past history is not easy and in some quarters the old charge of *Political Imperialism* has been replaced by a new one – *Cultural Imperialism* – a phrase so vague that it can mean whatever the accuser wants it to mean. It has nothing to do with the politics of Right or Left, but is primarily an economic phenomenon deriving from the concentration of wealth, information, technical resources and trained personnel in Europe and North America, made worse by the dominance of English as the language of scientific communication. This situation, in itself, puts the Third World scholar at a disadvantage through no fault of his own (Bray, 1985: 448), and when technological and financial power is used irresponsibly, as a means of interfering in the affairs of a less developed country in ways that are against its wishes and interests, there is bound to be resentment and hurt pride.

I do not accept, however, that the faults are all on one side. Over the years I have come to believe that 'Third Worldness' (like Imperialism) is as much an attitude of mind as an economic problem. It can take many forms, from a tendency to see the CIA, international capitalism or evangelical protestantism behind every foreign mission, to an equally unjustifiable belief that whatever is foreign must therefore be best. I remember, too, the comment by

one of my local co-directors after we had been working together for some time. 'I wanted to learn the new foreign techniques', she complained, 'but you do just the same as us – only you're better organised.' We agreed, too, that for the foreign archaeologist, with limited time and a tight budget, the bureaucratic chaos (still a national art form after 150 years of political independence) can look exactly like deliberate obstruction. It costs nothing to keep an appointment, or to answer a letter on time. That said, though, there is often a real difficulty in reconciling conflicting political needs and professional aspirations. Area-specialists based in Britain are usually *international* in their interests; we study Latin America, the Roman Empire, the prehistory of Europe, and so on. Our loyalties are not, and cannot be, exclusively to one country. In the developing world the situation is different. Archaeologists, and their administrative overlords, are usually concerned with the history of their own countries and rarely venture outside them. For a British scholar interested in, say, the origins of agriculture, an excavation abroad provides just one piece of an international jigsaw, but for his local co-director it is part of a different picture, the prehistory of his own country. As José Luis Lorenzo (1976: 29) has pointed out, there are in effect two separate archaeologies dealing with the same body of data.

This source of conflict has been analysed by another Latin American archaeologist, Carlos Ponce Sanguinés (1978a, 1978b), a former director of the Bolivian Instituto Nacional de Arqueología, which regulates, among other things, the activities of foreign researchers. His two papers give an unusually frank, and at times emotive, view of how a foreign presence may appear to an over-stretched and under-funded Third World institution. Ponce claims that foreign-dominated 'Neocolonial' archaeology results in an intellectual dependency every bit as damaging as economic dependency and that the powerful research institutions of the developed world behave in many ways like transnational companies. As classic dependency theory predicts, the underdeveloped world produces raw materials (in this case, archaeological data) for the industrial world to reshape (synthesise). The relationship is an unequal one. The major policy decisions (research priorities) are chosen to suit the interests of visiting foreign scholars; foreign ideas, techniques and attitudes are uncritically introduced without proper consideration of local needs; the national universities become mere transmitters of foreign knowledge. Not least, because they lack both the technical resources and the mystique of foreign 'know-how', the local archaeologists are regarded (and may come to regard themselves) as second rate. It is hardly surprising that some of them have become touchy and defensive.

There is enough truth in this analysis to make me feel very uneasy, for I can see no simple way of solving the problem. Bruce Trigger (1984) has shown that recent intellectual trends, especially the New Archaeology and its derivatives, are making the situation even worse. On the one hand, the New Archaeology offers a methodology which it feels everyone should adopt. But, on the other hand, the search for universal generalisations leaves no place in the scheme of things for 'national archaeologies'. In extreme cases, Third World countries are regarded as no more than sources of facts to prove one theory or another, a battleground where foreign archaeologists of different theologies fight among themselves.

There is another factor, too. In a perceptive article on the relationship between local

and foreign (U.S.) archaeologists in Mexico, Jaime Litvak (1985: 378) makes the following comment:

'... from the late 1950s the onslaught of salvage and zone maintenance had put Latin American, and especially Mexican, archaeology under a terrible strain. The difference between the American academic, with a long-term, unhurried, well financed project, and the Mexican archaeologists, trying to deal with emergencies without adequate means, was very obvious. It was the U.S. archaeologists who were publishing the important books and getting all the medals. The Latin Americans were, it seemed, just being used as information gatherers.'

All this makes it difficult for a scholar from the Third World to join the international club, though (contra Ponce) I do not believe there is a deliberate policy of rejection. As Professor Michael Day remarked at the close of the 1986 World Archaeological Congress in Southampton:

'The dominance of Europe and North America and the wholesale exclusion of many developing countries from the councils of world archaeology is clearly no longer acceptable to world archaeological opinion.' (quoted in Harris *et al.*, 1986: 21).

Those of us who work overseas (and who travel in order to learn, as well as to teach) will applaud those words.

It is at precisely this point that I part company with Ponce Sanguinés and the nationalistic school. In opposition to 'Neocolonialism', he advocates a return to 'Arqueología Nacional', an archaeology relevant to the development and material aspirations of the Bolivian people. It will emphasise the glorious past (a traditional remedy when things are going badly in the present), will be intellectually autonomous and 'without foreign inspiration'. It will also 'show that national archaeologists can, with a purified technique (*con depurada técnica*), carry out research work at the same or a higher level than their metropolitan colleagues', taking advantage of their local knowledge and thus demonstrating that 'foreign experts are neither indispensable nor infallible . . . and that they do not possess innate superiority, as some anti-national circles maintain.' (Ponce, 1978b: 9). Foreign archaeologists are to be allowed into the country, though they will not be allowed to dictate policy and all research will be tightly centralised under state control.

The depth of feeling is evident, and the remedies draconian, though other developing countries have moved in a similar direction. I believe this policy is the wrong one and that the dangers outweigh the advantages. Nationalistic archaeology can easily become both provincial and chauvinistic (Litvak, 1985). Also, as Ian Glover has just shown, once archaeology becomes a state monopoly it inevitably becomes politicised; what is regarded as the national interest (whether this be tourism, rescue needs, or pure propaganda) takes precedence over free enquiry. It is not only foreigners who will find this irksome; so, too, will many local scholars. When these conflicts break out, the usual tactic is to fall back on entrenched positions, one side insisting on the right to do whatever it likes with its own heritage, the other asserting that science has no frontiers and that the past is the universal heritage of mankind as a whole. All these arguments conceal some very shifty logic.

First of all, whose heritage is being investigated and does the modern nation state have good claim to it? Many of today's independent nations have boundaries that were drawn up quite arbitrarily by colonial occupiers, ignoring past history and modern ethnicity. Do white Australians and South Africans, or the descendants of Hispanic conquistadors, have a moral right to say what shall be done with the archaeological remains of the indigenous peoples? The Australian aborigines are now saying 'No' to white archaeologists and many Amerindian groups would maintain that the national governments of today are every bit as 'Colonial' as those of the pre-independence era. We are all, it seems, still Imperialists of one kind or another. On the other hand, the doctrine that Science is Universal is open to abuse; it clearly does not give archaeologists from the developed world an inalienable right to carry out fieldwork anywhere, on any problem, and whether the recipients like the idea or not.

To work as a guest in somebody else's country requires tact and good manners and, in the end, these personal qualities are often more important than ideologies. Sometimes they have been lacking, and the complaints of heavy-handed conduct are the same all over the Third World (cf. also Evans and Meggers, 1973).

Work by local scholars is ignored by visiting researchers, is not reviewed in international journals, is slow to get into the textbooks, and in some cases is simply disbelieved.

Foreign missions attempt to circumvent, rather than to collaborate with, the local representatives, do not communicate their results, forget to send copies of the publications, and do not send back material allowed out on loan.

Individual foreign archaeologists, with careers to make in their home countries, publish outside and in English and are reluctant to become involved with the academic and social life of the host country.

With a bit more sensitivity all these things are avoidable – the way of dealing with such crass behaviour is through education, not retaliation. It may settle old scores, but it is archaeologically unhelpful to levy punitive taxes on foreign missions, deny access to museum collections, or to turn down reasonable requests for study loans.

In summary, behind all the high-minded platitudes is a very simple belief, that the relationship between British archaeologists and their Third World colleagues must be based on honest dealing, mutual respect and a willingness by each side to understand and make allowances for the other's history and personal difficulties.

Here at the Institute of Archaeology we have always taken pride in belonging to an *international* community, one which carries out research all over the world, and offers an environment where archaeologists from all countries can meet and get to know each other. This bridge-building role is vital. It therefore follows that any governmental policy that puts obstacles in the way of such exchanges is bad for archaeology now, and also puts our long-term effectiveness at risk.

Much of what I have been saying was triggered off by a conversation several years ago with the Director of Antiquities of a Latin American republic; for the sake of discretion I will call it INCADORIA. In the late hours of a party we began to talk about the real problems with foreigners, the things that never appear in the official correspondence.

'How would you feel', he asked, 'if a boatload of INCADORIANS spent three months cruising up the Thames, and then returned to write the "definitive" study of British culture?' This, I thought, would probably be no sillier than some of the things we write about ourselves, but his second question was much more serious. 'What will happen', he enquired, 'if an INCADORIAN mission puts in an application to excavate Stonehenge or any comparable major British site?' One day INCADORIA will have its oil; the National University will create a department of European prehistory, staffed perhaps by people trained at this Institute and who feel that they have a right as scientists to investigate in their chosen area.

If that application ever comes (and I can foresee a day when it will), then British archaeology will be put to a test that confronts Third World countries several times each year. In spite of the sincerely held belief that Science Has No Frontiers, I predict that the decision will not be an easy one.

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The Institute of Archaeology and Field Archaeology

by P. L. DREWETT

The founder of the Institute of Archaeology, Sir Mortimer Wheeler, was first and foremost a field archaeologist. Thus when Sir Charles Peers, Chairman of the Management Committee addressed the Institute's opening ceremony in 1937 he stated that 'If the essential character of this Institute may be expressed in a word, it is that it is . . . a laboratory of archaeological science, wherein the archaeologist of the future may learn the essentials of his business' (Peers, 1938). Clearly foremost in his mind was the science of field archaeology, with its major component, scientific excavation. Thus, in the Second Annual Report of the Institute for 1938 we see a clear statement of where Sir Mortimer Wheeler saw field training. In the Report on Routine Work for the session 1937–8 we read, and I quote in full:

'A Technical Training'

This has been placed *at the head of the list* [my italics] for in this the Institute is to a large extent doing pioneer work. Most archaeologists today have acquired their training in a somewhat haphazard way, often by a rather painful experience of how not to do things; they have generally learnt by experiment, in which the subject of the experiment, whether a site or a particular find, has naturally suffered. Alternatively, directors of excavations and others have had to spend much of their time, already all too fully employed, in giving elementary instruction to students. It is the aim of the Institute to provide students with a thorough grounding in technical methods, though it will of course, always be necessary to supplement this by experience.'

It is revealing to see on the next page how these aims were implemented through the establishment of two courses, one on drawing and the other on field archaeology. If I may quote in full again, it is particularly revealing how the sixteen published lectures on the 'Principles and Techniques of Field Archaeology' may be seen as the basis of practically everything the Institute does today, although many single lectures are now either entire courses or in one or two cases even entire degrees:

'A course of lectures was given this year (1937–8), the first arranged specifically for this object, to give grounding in actual methods of excavation in the field. All aspects of the work, both methods of attack and the principles of recording results, preparing plans, sections, notes and photographs were covered.'

First Term

Archaeological Draftsmanship – A course of six lectures with demonstrations by Mr G. C. Dunning, FSA. Fee: £1 1s. 0d.

Second and Third Terms

The Principles and Technique of Field Archaeology. Fee: £2 12s. 6d.

1. General Principles: Buildings by Dr. R. E. M. Wheeler, FSA
2. Earthworks by Dr R. E. M. Wheeler
3. Records and Reports by Miss K. M. Kenyon, FSA
4. Principles of the Identification and Significance of Coins in Fieldwork by Mr B. H. St.J. O'Neill, FSA
5. Air Photography in Archaeology by Major G. W. G. Allen, FSA
- 6–7. Forest History and its Relevance to Archaeological Studies by Dr J. G. D. Clark, FSA
8. The Excavation of Caves by Mr. Leslie Armstrong, FSA
9. Maps and Physiography by Sir Cyril Fox, FSA
10. Animals and Bones by Dr Wilfred Jackson
- 11–12. The Surveying of an Archaeological Site by Mr Huntley Gordon, FSA and Mr H. O'N. Hecknen.
13. The Use of Samian Pottery in Roman Excavations by Dr. T. Davies Pryce, FSA
14. Primitive Techniques of Pottery Making by Mrs J. W. Crowfoot
15. Treatment of Objects in the Field by Miss I. Gedye
16. Excavation Methods and Organization in the Near East by Miss K. M. Kenyon

Field archaeology has, however, gone a long way since 1937. Not only are our fees for teaching field archaeology to overseas students some 1,423 times higher than in 1937, but also the basic approach to field archaeology and particularly excavation has changed. We read of ‘methods of attack’ as though the archaeological site was the enemy, to be chopped up with military precision. A grid of boxes was hammered through the site. Today we hope, on a good excavation, data is coaxed out of the ground rather than attacked but that is perhaps jumping a few decades.

Sir Mortimer Wheeler’s contribution to Field Archaeology through the establishment of the Institute of Archaeology and its Technical Training Programme is clear for all to see. There can have been few British archaeologists working in the 1940s and ’50s who did not benefit from it in some way. Great names on the Institute staff were made largely through fieldwork, or at least on the excavation of classic sites: Max Mallowan, Professor of Western Asiatic Archaeology excavating at Nimrud; Kathleen Kenyon, who was Acting Director through the Second World War, making her name at Jericho and Wheeler himself with Maiden Castle, Verulamium and his Indian adventures. It was a time, to quote Harrison Ford, of much ‘fame and glory’ for archaeologists. Field Archaeology in the 1950s became one of the

last great exploration adventures, akin to the discoveries of 'new worlds' by the 19th-century explorers or space travel in the 1960s. Leading this role of fieldwork in the public limelight was Wheeler 'whose frequent television appearance and extrovert personality made him a household name' (Green, 1981; 107).

The Directorship of the Institute during these heady days was, however, in the hands of Gordon Childe who would not have considered himself foremost a field archaeologist. Indeed, it has been claimed that his excavations at Skara Brae were the only ones he apparently enjoyed (Green, 1981, xxi). The excavation report on Skara Brae does, however, show a remarkable natural ability at excavation, although no doubt Wheeler would have considered it rather rough around the edges.

Field training, along the lines begun by Wheeler in 1937 continued throughout Childe's Directorship with M. B. Cookson teaching photography, Harry Stewart teaching surveying and drawing, and field archaeology proper being taught in part by most members of staff. In 1957 the Directorship of the Institute passed back into the hands of a great field archaeologist but one in a very different mould to Wheeler. This was Professor W. F. Grimes. Professor Grimes also had his 'fame and glory' sites like the Temple of Mithras in Walbrook, but this was more by accident than design. His field archaeology was particularly geared to the past of ordinary man and as such he was very much a field archaeologist of the 1960s. Would Wheeler, for example, have seen the value of recording the field systems on the little known Island of Caldey off West Wales? (Grimes, 1955; 85–165). Perhaps he might, but Grimes most certainly did. He also appreciated that data not recorded prior to building work in towns would be lost to archaeologists for ever. He was hence one of Britain's first rescue archaeologists, working particularly in the City of London.

It was during Professor Grimes' reign that I first came into contact with the Institute and field archaeology. The first was somewhat accidental, the second more by design. Neither were particularly encouraging, but at least I knew from the start that the Institute was where field archaeology happened. The first contact was in 1959 when, in answer to an adventure competition, I wrote to the *Boys Own Magazine* (now defunct) about my discovery of a Roman site in Croydon. Instead of publishing it, the letter was forwarded to the Institute. The reply made the position of small boys in archaeology amply clear! Fortunately my training in field archaeology got off to a far better start twenty-five years ago this year on Fyfield Down, with a youthful Peter Fowler, now Professor of Archaeology at Newcastle. This, in the Grimes tradition, was a landscape formed by ordinary people not set on fame and glory.

My second contact with the Institute came in the early days of my professional career in archaeology. In 1966 I came to University College to study Anthropology under Professor Darryl Forde. During our introductory week I came over to the Institute to attend the Institute Archaeological Society's Cheese and Wine Party. I was approached by the Secretary who asked me what subject I was doing: 'Anthropology', I replied. 'Oh', came the answer. End of conversation. Little did I think that six years later I would be back in the Institute to teach field archaeology. It is on this last 14 years of field archaeology and the Institute that I intend to concentrate, ending with some comments on the future.

I think it is fair to say that the teaching of field archaeology in the Institute in 1973 was

not entirely satisfactory. The field course had run into problems, largely based I think on inadequate funding and unsatisfactory teaching arrangements. Wheeler's all embracing 'Technical Training' of 1937 had fragmented somewhat, with separate drawing and surveying and photography courses. Excavation was taught by outside lecturers on non-Institute excavations. Non-excavation field archaeology appeared to be largely absent from the curriculum.

Both Wheeler and I went from University College into public archaeology, he to the Royal Commission on Historical Monuments in 1913, I to the Ministry of Public Building and Works in 1970. Interestingly enough, to me anyway, Wheeler's first job was to survey the Roman remains in Essex (Wheeler, 1955; 64), while mine was to schedule remains in Essex. I joined the Institute in 1974 under its new Director, Professor John Evans who, having undertaken classic excavations at Knossos and fieldwork in Malta, realised the importance of continued development in teaching field archaeology at the Institute.

My first two attempts at teaching field archaeology on the Institute's Easter Field Courses in 1974 and 1975 were an utter disaster. The reasons were complex but certainly the need to remove mounds of sand over 2 m high, containing no finds, under rescue conditions did not help (Drewett, 1976). Also a field course based on a single site out of its landscape context was not a sound way to introduce students to field archaeology.

The successful development of a Department of the Environment-funded rescue unit within the Institute during 1974–75 opened up new possibilities for field training. In 1974 the Institute took over responsibility for all publicly funded rescue archaeology in Sussex. This enabled us to develop research based rescue archaeology projects, one of which, the Bullock Down Project, presented an ideal opportunity for training in all aspects of field archaeology. From 1976 to 1984 Bullock Down remained the basis of the Easter Field Course, on which all undergraduate field training hung (Drewett, 1982a). The Department of the Environment funded the project until 1981 but fieldwork continued until 1984, while the actual training excavations took place close by at Selmeton in 1981 (Rudling, 1985) and Seaford Head in 1983 (Bedwin, 1985).

Bullock Down offered just about everything required for field training (Fig. 1). The courses were tied closely to landscape archaeology. Students were taught to see their sites as part of a developing landscape and not merely as discrete archaeological sites. We talked of the Roman Farm with fields, trackways, marl pits, manured fields, farm yards and pasture. The settlement became only a fragment of the whole (Drewett, 1982a, 4). Even the farm itself became part of the larger landscape. Where were the next farms? Was Bullock Down within an Eastbourne Villa Estate?

For eight years training in field archaeology on the Easter Field Course followed a set and highly successful formula. The Field Unit taught the identification and interpretation of earthworks, field walking (Fig. 2), and all aspects of excavation from removing the plough soil to drawing plans and sections. Visiting lecturers came from the Institute's Department of Human Environment. They put the landscape into a wider natural context and taught field techniques of environmental archaeology. Peter Dorrell and the Photographic Department taught on-site photography, while Harry Stewart taught surveying techniques. David Haddon-Reece from the Ancient Monuments Laboratory brought an endlessly developing range



Fig. 1 Landscape Archaeology at Bullock Down. This formed the basis for the Institute's Easter Field Course 1976-84.

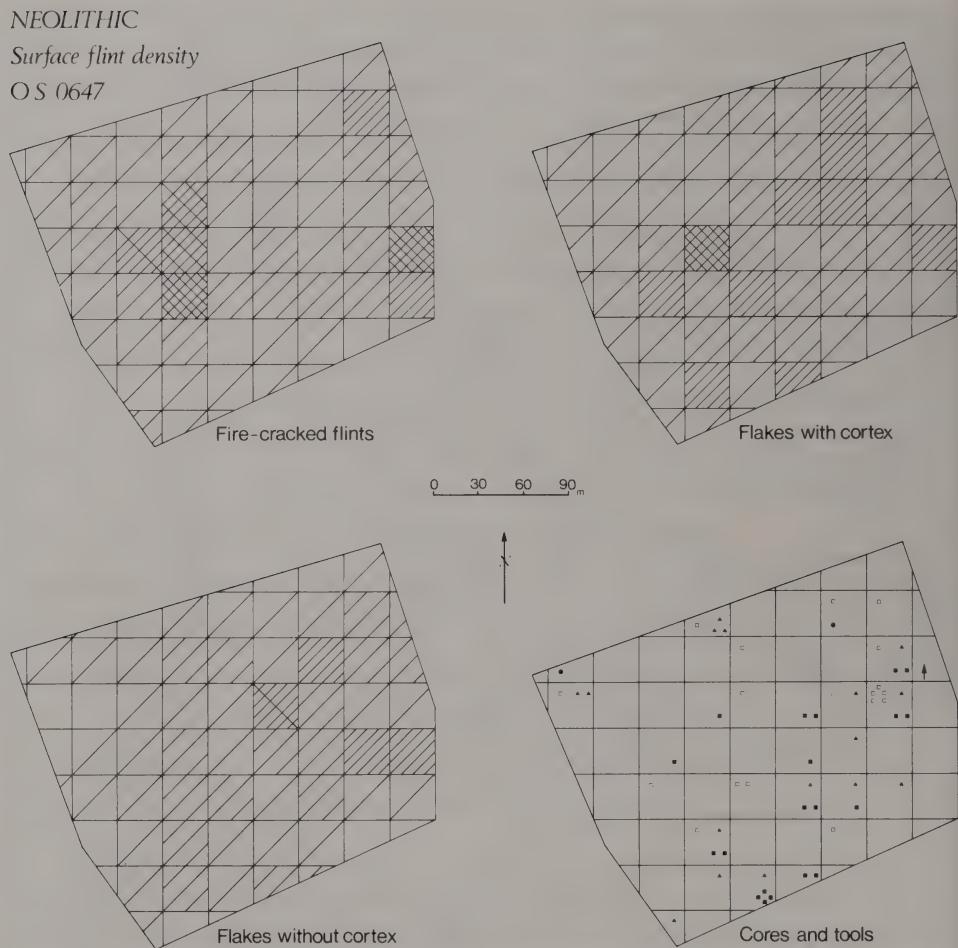


Fig. 2 Systematic field walking became one of the most widespread new techniques in the 1970s. This example was based on the Easter Field Course on Bullock Down.

of geophysical equipment which he demonstrated every year. An historical dimension was provided by the Mediaeval Archaeology Department from University College.

Running parallel to the highly successful Bullock Down Project and Easter Field Course were three other projects which provided students with a wide range of field experience during the summer vacations. I ran a series of excavations within a research based rescue project based on ceremony, settlement and territorial organization in Sussex during the third-second millennia BC. Key sites excavated included the Alfriston Oval Barrow (Plate 1) (Drewett, 1975); the Offham Neolithic Causewayed Enclosure (Drewett, 1977); the Black Patch Later



Plate 1 Institute students under training at the Alfriston Oval Barrow excavation, 1974.

Bronze Age Settlement (Drewett, 1982b); the West Heath Round Barrow Cemetery (Plate 2) (Drewett, 1976 and 1985), and the North Marden Oval Barrow (Drewett, 1986). Dr O. Bedwin, ran a similar project examining pre-Roman Iron Age settlement in Sussex with key sites including North Bersted (Bedwin and Pitts, 1978); the Devils Ditch; Boxgrove (Bedwin, 1982), and Oving (Bedwin and Holgate, 1985). Training in urban archaeology was not neglected with David Freke's Historic Towns in Sussex Project involving excavations in Lewes (Freke, 1975); Seaford (Freke, 1977), and Steyning (Freke, 1978).

In 1982, on the retirement of Mr H. Stewart as Lecturer in Surveying and Draughtsmanship, I was given responsibility for teaching his areas of study and so became the Institute's first Lecturer in Archaeological Field Techniques (the title including Prehistory and . . .). The Institute's Sussex Archaeological Field Unit, at the same time, changed its name to the Institute's Field Archaeology Unit. With a Lecturer in Archaeological Field Techniques and a new Unit, the Institute was able to start developing the teaching of field archaeology. A basic introduction to field archaeology, very much along the lines envisaged by Sir Mortimer Wheeler in 1937, is provided for all first year students, offering archaeology to all Departments of London University. This consists of a 20 lecture 'Introduction to Archaeology and its Methods' course, short courses in archaeological drawing, surveying and photography and a



Plate 2 Institute students under training on Barrow VI at West Heath, 1980.

10 day Easter Field Course. Since 1985 this course has had to be independent of Central Government funding and so has gone to the private sector with a research excavation based on the privately owned Lewes Castle (Plate 4). These excavations are geared towards the interpretation of the Castle as a tourist attraction. However, it does make an ideal training excavation, although now with a strong urban bias rather than the rural one provided by Bullock Down.

More advanced training in field archaeology was introduced in 1982 with the introduction of a course in Archaeological Field Techniques for undergraduates closely followed by an MA in Field and Analytical Techniques in Archaeology. The Field Techniques section of the MA course includes a study of archaeological field data, their origins and transformation, the design of field projects, organization and the law, archaeological surveys, pedestrian tactics and remote sensing, archaeological excavation and recording systems, draughtsmanship and the preparation of archaeological reports and archives. During this 12 month MA course students must spend a minimum of 70 days in the field. The Analytical Techniques element of the course provides the scientific background for archaeological methodology, with a strong emphasis on quantification and computer techniques. The course includes the scientific basis of conservation techniques, archaeological statistics, database management and computer



Plate 3 Making a video for field training on the Preseli Mountains Survey, 1985.

programming. Hopefully it produces students with, at least, an introduction to all techniques a field archaeologist will require who will still be working in the twenty-first century.

Interest in our MA in Field Archaeology has been truly international, with students applying from Africa, Hong Kong, Fiji, Jamaica and Peru, as well as the USA, Greece and Britain. One problem I soon encountered was that I was teaching techniques strictly based on lowland European situations. Techniques applicable to the Highland Zone and more particularly to tropical climates, are very different. After all, what use is a consideration of crop marks in wheat or grass if your country only has bare rock, maize or sugar cane!

I therefore felt that projects must be available both in the Highland Zone and the tropics. Working against such a development was, however, the cost. An invitation from the Barbados Museum, through Dr Warwick Bray, to undertake a survey of Prehistoric Barbados was therefore an extremely welcome proposition. For two winters I have been able to take out teams of students to work on tropical sites at first hand (Fig. 3). This project has also provided me with valuable experience, teaching videos and slides for future courses. The Highland Zone teaching was solved by starting a survey project in the Preseli Mountains in West Wales (Plate 3 and Fig. 4). This is an area I knew well, having first dug there with John Lewis of the National Museum of Wales in 1965 (Lewis, 1974).

The training of field archaeologists at the Institute of Archaeology is now, I think, more

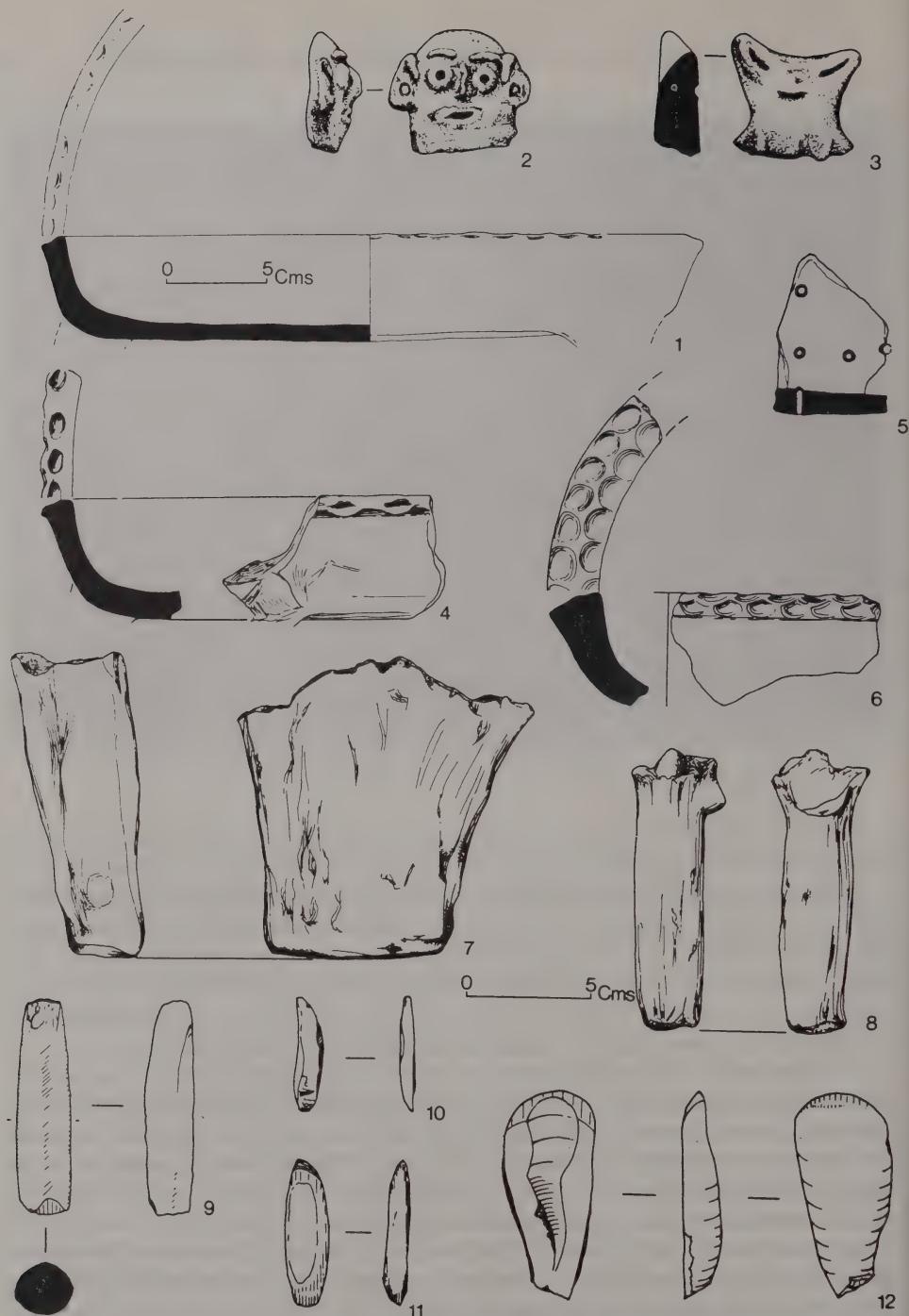


Fig. 3 Archaeological drawing. An example of illustrations produced in the field by the Institute's field team surveying Prehistoric Barbados, 1986. The Suazoid pottery and shell tools were excavated by the team at Heywoods.



Plate 4 Institute students on the 1985 Easter Field Course at Lewes Castle.

firmly established than ever before. There are, however, a few problems that remain. The most worrying one is that, with the exception of my salary, practically the whole field archaeology training programme is run on what our University administrators delightfully refer to as 'soft money'. Those of us who earn such money find the process anything but soft! Secondly, there is a curious breed of archaeologist who sometimes refer to themselves as 'academic' archaeologists (as opposed to 'field' archaeologists) who still consider field archaeologists somehow rather inferior technicians. This is rather like surgeons being considered inferior to physicians simply because their art (or science) is essentially practical. The results of bad excavation are, of course, clear for all to see; the results of bad theoretical or synthetic archaeology are usually hidden behind appalling jargon and pretentious titles in obscure journals. If archaeology moves too far from its database, which is field archaeology, it could become just another, second rate, form of sociology. Field archaeology and 'academic' archaeology (often seen in this country as University as opposed to Unit archaeology) must get back together. At the Institute we are together, but resources are not yet evenly spread.

So what of the future? First let us consider the Field Archaeology Unit which provides the practical basis of our Field Training Programme. It is a good Unit which produces results. Like all such Units around the country it has to adapt to changing circumstances. No longer can it expect full central government funding. Effective cultural resource management, particularly at the County Planning level should mean that more sites can be protected rather



Fig. 4 A plan of a complex Bronze Age–post Roman site at Carnalw, Preseli, surveyed by second-year Institute students in 1983.

than excavated. However, sites can only be protected if they have been located, so extensive and intensive regional surveys must be seen as a priority during the next decade. Also there must be a 'public will' to support preservation. Public education is an essential part of cultural resource management. The Unit must therefore present a much higher public profile in the areas in which it works. This will also aid in raising private sponsorship for projects. Sponsors require a high profile.

The changing emphasis on what field archaeologists actually do must be reflected in what they are taught. The only growth area in terms of the employment of archaeology graduates appears to be in cultural resource management. The complexities of this career are

enormous. Cultural Resource Managers need to be archaeologists, lawyers, managers, accountants, educationalists, politicians and labourers all rolled into one. Currently this country has no courses to train archaeologists in these skills and their appropriate application. Can we, with our dwindling archaeological resources, afford to allow any more learning on the job? If we do, will the twenty-first century have any archaeological resources left for academic study? Should the Institute take a lead in this and establish the first MA course in Cultural Resource Management? The role of the Field Archaeologist in society is rapidly changing. Our training, both conceptual and practical, must and will change with it.

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A Bloomsbury Pharos: The Institute and Nautical Archaeology

by MARK REDKNAP and ANGELA CROOME

In addition to its role as a 'laboratory of archaeological science', the Institute was created to provide a home for teaching and research in 'those spreading provinces of archaeology for which no adequate provision exists at present in London or, in many cases, elsewhere . . .', 'to extend geographical horizons', and 'to extend technical elaboration of the methods of archaeological research'. Nautical archaeology forms one of those 'spreading provinces of archaeology' developing from antiquarian and commercial interests only after 1945 when the Cousteau-Gagnan aqualung made it possible for scientists and explorers to work cheaply, easily and safely in waters up to 50 m deep. For an innovative and immature subject dependent on collaboration between many related fields, the Institute has provided a concourse and guiding beacon.

Choppy waters: the early years (A.C.)

The Institute was brought into underwater archaeology by its first and long-time Librarian, Joan du Plat Taylor, and largely sustained in a dominant role in this new and demanding branch of archaeology by her. This applied even after her retirement (1970), for she became the *International Journal of Nautical Archaeology*'s first editor (1972–1980) and she held an 'elder statesman' position with the Council for Nautical Archaeology (CNA) and its affiliates – focal point of nautical archaeology for a generation – until her death in 1983.

Joan Taylor had built up both a countrywide and an international web of contacts and of informed allies more or less linked with the Institute through the CNA (brought into existence formally as the Committee in 1964) with its several practical thrusts. These included publication, law reform, training and, in particular, an educational, liaison and promotional role. This was perhaps the more remarkable since Joan was not a diver nor notably nautical – though she had been a key member (records and conservation) of the pioneering five-nation expedition to the Bronze Age wreck off Cape Gelidonya, Turkey, in 1960, which effectively put nautical archaeology on the academic map. Her lack of underwater experience was, however, more than compensated for. She was a tireless arranger of conferences and orientation talks; she was always busy editing and producing papers and reports, most notably the volume *Marine Archaeology* (1965) that she edited for CMAS based on the studies collected in *Le Plongeur et l'Archéologie*. Above all, she had a formidable talent for delegating. She could be relied on to

ferret out anyone with an interest however tangential in nautical archaeology and to harness these diverse skills and connections to use. The early CNA was a mission of all talents. It was also a focus of interest wherever the subject surfaced abroad. Almost anyone with a project in nautical archaeology from Fremantle to Restigouche, Fort Jesus to Port Royal, would sooner or later turn up in Joan's office at the Institute Library. Her missionaries sped to the ends of the earth – and the bottom of the sea – to spread the word. The 'word' of course was that archaeology was indivisible and that the same standards – and constraints – applied whether a site or an excavation lay on land or underwater, or, half-and-half as with port and harbour-works.

At this period it was indeed an uphill task – even in such an archaeologically literate community as England. On the one hand, the scholarly world was largely uninterested or actively hostile: 'just another excuse for a Mediterranean beach holiday'; 'no worthwhile research is ever going to be performed by divers. . .'. The rest of the population considered goodies pilfered from ancient wrecks justified reward for the dangers and discomforts of the frogman's lot (the diver still swam in an aura of 'Cockleshell Hero'). The Mediterranean was the cradle of underwater archaeology and the French were still leading exponents – and theorists – of the new technique and its possibilities, developed there in the 1950s following the wartime invention of the aqualung. The whole subject had a strong Mediterranean bias and the few British diving archaeologists, such as Honor Frost, had worked with or were influenced by French expertise.

In autumn 1962, the premier international meeting the *Confédération Mondiale des Activités Subaquatiques* (CMAS or 'World Underwater Federation') met in London and, for the first time, perhaps 50% of the papers dealt with sites outside the Mediterranean or were given by speakers who were not French. The *Wasa* had been lifted complete out of Stockholm harbour the year before. I had just returned from the excavation of the Skudelev ships in Roskilde fjord which I had covered for *The Daily Telegraph* and had an introductory book on archaeological wrecks in hand. As the conference assembled, Peter Marsden was completing the 'rescue' work on the Roman period barge discovered off Blackfriars Bridge during foundation work for the new underpass; a medieval cog in a good state of preservation had been located in the river Bremen; The *National Geographic Magazine* had sponsored an expedition to Tristan da Cunha to find the sunken remains of the *Bounty* and this was reported on at the conference.

The Institute acted as host for a number of the conference participants and this was my and several other people's introduction to Joan and the budding CNA and the activities of the Institute's Underwater Research Group. Apart from Joan herself and Dr John d'Arcy Waechter most of the people involved in the early informal meetings in Joan's office at the Institute came from outside, some of the more senior being connected with the Society for Nautical Research (SNR), the parent body of the National Maritime Museum with its own well-established journal, the *Mariner's Mirror*. Honor Frost was connected with the Institute through her work as draughtsman on some of Kathleen Kenyon's excavations. She had just brought out her translation of Frederick Dumas's seminal book and her own book *Under the Mediterranean* was in the press (published 1963). Cast in the form of highly entertaining

memoirs it traced the steps from Turkish sponge-divers' finds of copper ingots to the most sophisticated underwater survey yet mounted.

George Naish, the Keeper of Ships at the National Maritime Museum and secretary of the SNR, was a walking encyclopaedia on matters maritime and fount of information and enthusiasm; also a passionate yachtsman, he had embraced the underwater world and appreciated its potential in a way that shamed many scholarly and nautical colleagues. He was soon to be the CNA's first chairman. There were several museum men including the Keeper of Guns at the Armouries, Tower of London; a marine lawyer, and serving and ex-service officers with diving experience came and went. The British Sub-Aqua Club and the CBA were represented from the beginning. In the wings were several bright young men from Cambridge who were extending the scope of their research through diving. Nic Flemming was mapping the sea-level changes of the early Mediterranean basin. The pattern of ancient ports and harbours in the same area absorbed Bob Yorke and David Davidson. Classicist and linguist David Blackman was interested in classical docks and the ships that fitted into them.

Peter Marsden has elsewhere given an account of how Joan's informal meetings gravitated in 1964 into becoming a regular research group, the Committee for Nautical Archaeology (Marsden, 1986). Peter's main preoccupation lay with ship remains on land and it was to bring the vulnerability of boat finds to public notice that he sent out the circular which led to the CNA's formation. Nevertheless, for some years CNA functioned principally as a talking shop, as before, but the Institute became increasingly the focus of nautical archaeological interest and enquiries were addressed there; it was on the CNA letterhead.

There were some week-end conferences. Joan organised regional representatives as contacts up and down the country for divers with local problems or projects. CNA also acted as a vetting agency for expeditions going abroad; they had a need for responsible endorsement for their credentials to obtain permits, licences and so forth in the host country. It was also the reporting point for projects in Britain, on many of which the committee advised (at its peak the number of expeditions per session was in double figures). There was always a CNA presence at the two annual conferences in the underwater calendar – the spring meeting of the Underwater Association and the summer meeting usually held at Brighton organised by the British Sub-Aqua Club. During one of these attended by the American pioneer in the field Peter Throckmorton (1966), Joan arranged a day's outing in the Solent at which Peter was to give his underwater assessment of the *Mary Rose*'s chances; the site had yet to be located. His test dive produced the classic judgement 'what marvellous mud for an airlift!' – so pervasive that even a few feet below the surface it was impossible to tell which way was up, but as a preservative – 'the *Mary Rose* is likely to be intact from at least the gundeck down. You will get a complete cross-section of Tudor life from her contents. Since she must be lying in clay everything will be there. . .'. He was cannily correct.

Another field of investigation of this period was a re-survey of the *Grace Dieu* of 1418, buried in mud of the tidal Hamble River above Burlesdon Bridge. The opportunity of exceptionally low spring tides in September 1967 briefly uncovered the remains of Henry V's giant warship. The Revd F. C. P. Naish had established the identity of the wreck in 1933, supported by a survey by a young officer of the Royal Engineers, Michael Prynne (Anderson,

1934; Prynne, 1968, 115–28). Marginally smaller than Nelson's *Victory*, she burnt down to the waterline in 1439 after being struck by lightning. As the ebb tide swirled away in the late afternoon of 7 September, Major-General Michael Prynne (as he now was), accompanied by Professor Peter Morice of Southampton University, arrived by appointment with the CNA chairman to see the outline of the burnt timber heads emerging (Plate 1). Major-General Prynne was delighted to find the 300 ft hull in much the same condition as when he had measured it 14 years before, and later devoted a Society for Nautical Research paper to suggestions for her preservation and display. She became an early candidate for designation under the Protection of Wrecks Act (1973) but, tragically, Michael Prynne was killed in a motor accident at about the same time, and George Naish only survived him by four years, so the plan has faltered.

Meantime discussion continued at the CNA meeting, much of it centering on whether archaeologists should turn diver or divers acquire archaeological grounding; there were not enough of either to go round. Every possibility was taken to inculcate the doctrine that 'treasure' was a dirty word and that excavation must not be spelt 'salvage'. Nevertheless, when reality broke in the CNA was both scattered and helpless with all the archaeological members away. This was midsummer 1967, when a Naval Air Command diving group, as part of a training exercise, located what was claimed to be the wreck of the *Association*, Admiral Sir Cloudesley Shovell's flagship sunk in the western approaches of the Scilly Islands while returning with considerable booty from the Mediterranean in 1707.

We learnt a lot about the shortcomings of the law that summer. The site was not conclusively identified as that of the *Association* but the naval group were 'salvors in possession' which gave them substantial status, except that after a fortnight or so their exercise was over – and so was any sort of protection they could give to the site; the news of the finds had already been 'blown' and a fine bronze cannon landed. A free-for-all developed. Diving freebooters homed in on the Scilly Islands from all over the kingdom. The extremely exposed diving area acted as the most effective shield but it was not enough: an exceptionally fine summer was not on the side of careful, orderly excavation. It emerged that no less than three groups (including the Naval Air Command Sub-Aqua Club) had been awarded salvage licences for the *Association* by the Ministry of Defence (MoD) – an invitation to rivalry, confusion and selective recovery of the most valuable items.

The press was the only immediate weapon. The CNA chairman wrote a letter to *The Times* expressing concern over the activities of 'treasure-seeking divers' prepared to use explosives which 'will cause irreparable damage to wrecks of great historical interest. In the Mediterranean old wrecks are fully protected but in British waters only ordinary salvage laws apply'. In August the MoD banned explosives and warned the Morris team that its contract would be reviewed if they were used once again. The underwater landscape, rendered 'unrecognisable with broken iron cannon all over the place' attracted plenty of attention and a meeting between CNA representatives and the MoD and Board of Trade (BoT) took place in late 1967. As a result the MoD offered to inform the CNA when it was 'handing out' salvage contracts for naval vessels sunk before 1870 and, in effect, ceased to give multiple contracts for the same wrecks. The BoT was to ask Receivers of Wreck to communicate with CNA if what



Plate 1 *Grace Dieu* re-survey 1967. Reading L-R: Professor Morice, George Naish, Mrs Morice, Maj. Gen. Prynne (with object), A. McKee (with camera). Photo: A. Croome.

appeared to be historic wreck remains were reported and, in the coming two or three seasons, some were.

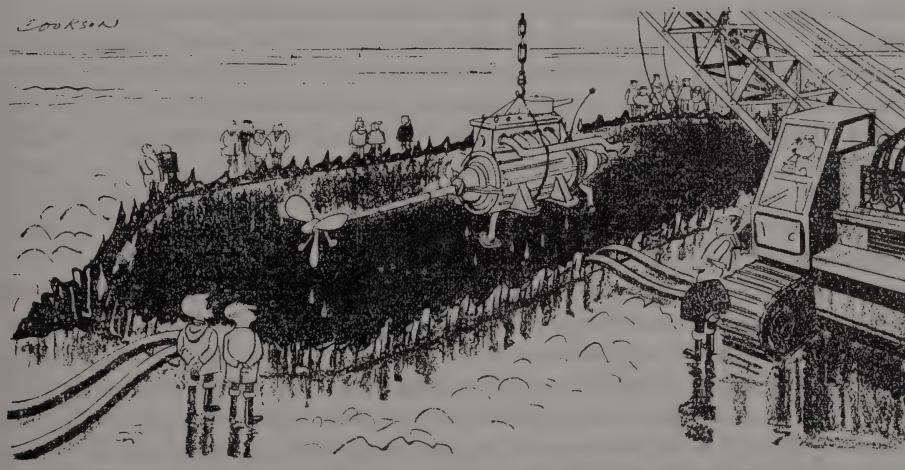
It was soon clear that neither measure, though well meant, was going to be sufficient. In particular the case of the *Mary Rose* was a worry; on the threshold of positive identification of the site, the Mary Rose (1967) Committee had been formed, but feared to take practical steps until some enforceable protection and title could be obtained. An exclusive MoD contract was one possibility. The committee chose an alternative approach – the lease of a defined 1200 square feet of seabed (on the analogy of North Sea gas blocks) and the Crown Estate Commissioners awarded the lease in spring 1968 for the sum of one pound per annum – a useful precedent as it was thought at the time.

Meantime in the *Association* affair: two ships had been lost by diving groups; three libel cases and one High Court injunction had been filed; further dynamiting had done widespread damage (the MoD had no control over non-contractors) and the first auction of historic wreck finds, at Sotheby's on 14 July 1969, had generated absurdly inflated prices and distorted the market. There had been a Crown prosecution over a pair of 18th century dividers from a Scilly Islands wreck which had failed – a discouraging precedent. This accumulation of scandals could not be ignored. In due course they provided material for much eloquence in Parliament in moves to improve the law and as irrefutable arguments that the British were the 'philistines of Europe' in such matters.

Furthermore *Romney*, another wreck of the 1707 Shovell squadron, had been traced; there was active searching and a contract out for HMS *Colossus* carrying the famous Hamilton collection of classical antiquities, and another group were on to a rich East Indiaman, thus promising to maintain the reputation of Britain's key ship graveyard as an archaeological disaster area.

These events smartly altered CNA priorities. Reforming the law now became the prime objective. The CNA had considered the possibilities of obtaining protection for ancient wrecks under existing law and probed its ineffectiveness. Now systematic study of the law's provisions and how it might best be amended or redrafted to meet the modern problems was put in the hands of legal counsel. In parallel an enquiry was instituted into the operation of laws to protect wreck and archaeological sites in foreign countries that had introduced modern measures. Both studies were ready in 1969 and were soon drawn upon when John Nott, MP for St Ives division of Cornwall, having obtained leave to introduce a Private Member's Bill in Parliament under the ten-Minute Rule, sought the advice of the CNA. In fact CNA provided Mr Nott with a full briefing and its legal member, Tony Miller, drafted the Bill which he presented on 4 March 1970. This was well received though, in common with all such Bills, had no chance of becoming law, so CNA's advisers redrafted a measure which Mr Nott subsequently presented as an amendment to the Merchant Shipping Act (1894), part of which was then being revised in Parliament. This could not be accepted by the Government but it was sufficiently sympathetic to look at the matter further. A combination of factors enabled the CNA to make the most of this situation and the study group set up by the Board of Trade in April 1970 had the merit that it was unaffected by the June election which would have killed any law still pending. These factors were as follows.

In Summer 1969, during work on a sewage outfall on Hastings beach, the contractors made use of heavy equipment (normally banned from the beach) to dig into the hull and cargo of a Dutch East Indiaman buried in quicksands since 1749. The CNA was alerted and a survey led by Peter Marsden, Bill Wilkes and Lt-Commander Alan Bax was organised starting in September at equinoctial low water springs with the help of Hy-mac pumps and digger, and the BBC *Chronicle* programme. Fourteen extra-low tides were needed to complete the survey and, on the last, 10 March 1970, the CNA arranged a press call for Dutch and English journalists which was billed 'for dawn on Hastings beach – bring gumboots'. John Nott's 4 March speech was used as a background handout. The visit was a spectacular success and the next morning's papers had full-page pictures of the *Amsterdam* surrounded by waterproofed visitors and scientists (Plate 2). In a miniature debate that afternoon John Nott made good use of the publicity to contrast the treatment of the *Amsterdam* under CNA auspices with the rummaging of the *Association* and called for action from the Minister of State for Shipping, Goronwy Roberts. Under continued CNA pressure the Board of Trade set up in April an interdepartmental committee with the CNA and other outside membership to consider changes in the law of wreck 'in the light of modern conditions' and to make recommendations. At the first meeting in July, CNA had four representatives, including Professor Grimes, then Director of the Institute. It was a great asset in these deliberations that Alex Flinder (representing the British Sub-Aqua Club on the CNA) was able to speak for the divers so that archaeology and divers – untypically – presented a united front on almost all occasions on the committee. Immense effort had gone into obtaining this one small step but there was no further meeting of the Board of Trade Wreck Committee for 18 months. It may be said that



"In purely archaeological terms, Henderson, I can only describe it as bloody fantastic!"

Plate 2 Cartoon taken from *Evening News* (11.3.1970) that appeared the day after the press visit and feasibility survey of the *Amsterdam* and on the same day as John Nott's speech in the Commons.

before the *Association*, the CNA was an unknown body; by the end of 1970 it was being constantly quoted in and out of Parliament, often taking precedence over the National Maritime Museum.

The survey and care of the unique *Amsterdam* was the achievement of the CNA as a group. Through speedy action a general pillage had been prevented, a great deal of information obtained, a thorough record of a surprisingly complete hull and cargo had been made and the co-operation of the Dutch government in launching a joint rescue operation achieved. This was of significance as a venture in European cooperation as well as for Britain. Without CNA's continuing care and watchfulness it was likely that the Dutch government would have missed the chance to register its claim to the wreck – it did this with a day to spare – and thereby all rights of protection until a thorough examination was possible.

To improve communication an annual newsletter had been started in 1967, and ran to four issues with about 1000 copies being distributed. It reflected the growth in CNA activity in this period which bracketed an active phase of the Institute's Underwater Group under Helena Wylde's presidency; the planning of the *International Journal of Nautical Archaeology* (*IJNA*); the launch of an appeal for funds, and the formation of a charitable trust to help finance activities on a more permanent basis. The trust came in handy for the *Mary Rose* project (by now independent of CNA support) which received a donation from the Duke of Edinburgh before it had obtained its own trust status. The NAT's most notable achievement was the Armada symposium held at Canterbury in spring 1974 where interpretations of the new material arising from the study of actual wreck sites (such as the *Girona*, *Santa Maria de la Rosa*, *Trinidad Valençera* and *El Gran Grifon*) were presented for the first time.

Planning for the *Journal* now absorbed the energies of several CNA members. To start with Honor Frost took the lead. She had found a suitable and interested publisher in the Seminar Press Ltd. with a stable of American and British scientific journals and a London office. Joan Taylor, who was retiring from the Institute in 1970, was editor-designate. I had succeeded her as CNA honorary secretary with Heather Bell, now Institute Librarian, as deputy (so maintaining continuity at the Institute). As Joan was away digging in Italy until autumn 1971, I was acting editor for this period. The preparatory team which met in Honor's flat during the winter 1970–1 included David Blackman as well as myself. A first annual volume of 250 pages was launched in conjunction with a major CNA exhibit at the Brighton International Conference 'Oceanology International 72' and immediately became the principal organ of publication in this field.

The next 'incident' lay largely outside the CNA's scope but produced many recruits for nautical archaeology. The newly appointed Director of the National Maritime Museum, Basil Greenhill, took the Graveney boat firmly under his wing. An intense wintry fortnight in late 1970 led the NMM into its first serious plunge into ship archaeology, followed by the setting up of the first national centre with comprehensive wet-wood conservation facilities, a ship archaeology gallery, the appointment of an archaeologist to the Museum staff at Assistant Keeper level and a wide-ranging series of symposia and publications. Much of these, alas, under threat of being swept away with the latest new broom so that the NMM is almost as innocent of archaeology as it was in 1967.

Events in summer 1971 finally triggered moves that led directly to the changes in the law that emerged as the Protection of Wrecks Act (1973).

On the same day two distinct amateur diving groups from Merseyside came upon opposite ends of the royal yacht *Mary* sunk off Anglesey in 1675 – the first yacht to sail in British waters, and the gift of the Dutch to Charles II at his Restoration in 1660. Fine bronze cannon were recovered and then disappeared. Despite the prompt intervention of the CNA's north-west representative, Dr Peter Davies of Liverpool University, who persuaded the two groups to work together and to make predisturbance surveys, more 'goodies' were ripped up and disappeared, fighting broke out, explosives were used with divers in the water, and a naval auxiliary was dispatched to try and keep the peace. CNA secretary and Peter Davies appealed urgently to the MoD to issue an exclusive licence to the now official expedition and to the DTI to institute proceedings over the theft of four fine English bronze cannon. The local press carried banner headlines and, on holiday in Wales, Professor Grimes was appalled by what he read. He was not the only one. With a keen yachtsman at No. 10 Downing Street and another in Buckingham Palace it seemed impossible that nothing could be done. What had happened to the Wreck Committee? With its work barely started (after an 18-month delay) was it really stuck with a five-year programme and a comprehensive revision of the entire law concerned with wreck and salvage? Prompted by these thoughts I, as CNA secretary, wrote to the Prime Minister Mr Edward Heath, drawing attention to the recurring scandals created by the outdated salvage law where anyone who could dive (and there were now 15,000 registered amateurs) could compete for treasure from ancient wrecks; that this caused lawlessness and violence each summer sometimes involving the aggressive use of firearms and explosives; that it negated the interests of archaeologists, art historians and marine scholars; and, that there was a public interest in the proper preservation of such material. The DTI had refused to act (under current salvage law) on goods raised from the *Mary* but not delivered up. The Wreck law review body had met only once and was only now reconvening under pressure. The letter ended: 'May we seek your support in protecting our uniquely rich and vanishing maritime heritage? All other European countries have already done so . . .'. The letter was sent on 22 October; it was acknowledged by the Prime Minister's office on 26 October and a detailed answer received a few days later from the DTI Parliamentary Under Secretary of State (Anthony Grant) proposing that interim legislation for historic wreck protection by way of a short Bill should be discussed at the 4 November Wreck Committee. In fact at this meeting (where CNA representation had been strengthened) the principal points for interim legislation were agreed on and sent to Ministers for approval. The Wreck Committee met again on 22 December 1971, when we learnt that the proposals were being considered by Ministers on the same day. Not until March did we hear that Ministers had agreed to the preparation of a short Bill to give protection to specified wrecks, a draft was being prepared by Parliamentary Counsel and an MP had agreed to introduce the Bill under the ten-Minute Rule.

HRH The Duke of Edinburgh had for some time interested himself in the diving world (as well as being patron for the SNR and a trustee of the NMM). He had instituted an annual diving award that had several times been won for an archaeological project and most recently by Sydney Wignall, leader of the expedition which had located the Armada ship *Santa Maria*

de al Rosa in Dingle Bay in 1969 (there had been an attack on the expedition boat and an injunction issued by the Irish Court to restrain rivals). On the eve of Wignall's award in November 1971, the Duke sought his advice on a short and swift measure to speed up historic wreck protection. The documents landed on Whitehall desks at about the same time that the CNA's letter reached the Prime Minister. But the Christmas present for nautical archaeologists that seemed so close in December 1971 was to be postponed yet again.

Soon after the 8 March 1972 Wreck Committee it once more became clear that moves for law reform had declined into the doldrums. An interested party had persuaded the sponsoring MP to withdraw. The Navy department not only declined an exclusive contract for the *Mary* – to put it at less risk in the '72 season, but it proposed to distribute the matching bronze guns to half a dozen scattered institutions. 'It sounds like the distribution of loot from a 19th century barrow-robbing house party' exclaimed one outraged archaeologist. A co-ordinated campaign for Parliamentary and press publicity was instituted with the help of David Blackman's political contacts and the secretary's press connections. Professor Grimes was drawn more closely into the campaign and signed another letter to *The Times* on behalf of the CNA. From March 1973 he took over as CNA chairman for the final push for interim legislation. A letter was circulated to a number of concerned MPs seeking their support for a new initiative with joint party sponsorship. These included David Owen, Roy Mason, Tam Dalyell and Cranley Onslow. William Deedes MP, who was soon to become editor of *The Daily Telegraph*, was active behind the scenes and acted as liaison with William Hamling MP, a trustee of the NMM with a Woolwich constituency who was prepared to promote a new Bill with all-party backing. These efforts had their effect and by winter an amended version of the 'short Bill' was on the road again. It gained the key second reading unopposed on 2 March 1973 and became law on 18 July. In due course Professor Grimes was invited on to the advisory committee formed under Lord Runciman to assist the Minister in assigning licences for historic wreck under the new Act, and continued to serve on it after he retired and up till 1986 in his own right. It is a matter of remark that the Institute has otherwise not been represented on the advisory committee.

This closed a significant chapter for nautical archaeology in Britain for the CNA and for myself. My spell as CNA secretary was over the following winter and the affairs of the CNA – and of nautical archaeology – became increasingly dispersed. The CNA's last connection with the Institute ceased with Heather Bell's retirement as librarian at Christmas 1985. Yet the Protection of Wrecks Act (1973) should have been more a beginning than an end. It was after all, only 'interim' legislation. There was so much more to do.

The later years: changing tides (M.R.)

Many of the themes running through the early narrative continue as main threads through recent developments in nautical archaeology. Over twenty years ago Joan Taylor recognised the need for greater collaboration between diver and archaeologist in studying the marine environment: her publication on marine archaeology became a definitive statement that has only recently been superseded (Taylor, 1965). John d'Arcy Waechter's lectureship in the

Prehistoric Department (1957–78) was punctuated with similar awareness of the potential of work underwater. His research into land submergence and marine transgression resulted in 1962 in a four-man diving team from the Universities of London and Cambridge undertaking the survey for early submerged shorelines and archaeological sites off the coast of Gibraltar. This was a logical development of his work between 1948–54 in excavating the prehistoric Goresham's Cave at the base of a cliff on the east side of Gibraltar (Waechter, 1951, 83f.). The Gibraltar expedition had been concerned with two problems: the identification and plotting of submerged shorelines and the investigation of submarine caves. The experimental techniques using continuous recording echo-sounders have subsequently been employed with considerable success elsewhere.

Over the last twenty-five years the Institute's Underwater Research Group has played an important role in promoting nautical archaeology amongst undergraduate and graduate archaeologists. Joan du Plat Taylor convened the first meeting at the Institute in October 1961 with a lecture on land and sea archaeology and the indivisibility of the two fields; the subsequent series of lectures covered caves and sea-levels, photography, ships, ports, trade, marine growth and conservation. Martini-Rossi supported an archaeological expedition to Malta in 1962 with Joan's assistance, and these early years saw Institute students under direction from Brian Booth, National Coach for the British Sub-Aqua Club, undertaking pool training courses at Dolphin Square. In 1965 John Waechter established a training school in underwater archaeological techniques at Swanage in conjunction with the BSAC which opened on 30 May with six divers including Margot Varese, the registrar's assistant, submerging to a seabed class at a depth of 20 ft (Plates 3–5). The Swanage trips may have been regarded by some as Dr Waechter's 'follies', but he had a very demanding approach. His concept of training divers and archaeologists to record underwater in a classroom on a submerged wartime pontoon carrying broken chimney pots, tiles and water jugs has now been developed by the Nautical Archaeology Society as part of the three-part Certificate in Underwater Archaeology.

An important objective of the URG remained the promotion of nautical archaeology amongst students of its parent discipline, a role the Institute continues to play. Pool and open-air training and lectures became a regular part of extra-curricula activities for members and associates: the lecture programme for 1967 included subjects covering 'Medieval Ships' (W. Batho); 'Underwater Archaeology in Sea, Lake and Ditch in Italy, 1965' (H. Wylde); 'Ancient Ports and Harbours in England' (B. M. Beeby); 'Litter on the Sea-bed' (G. Taylor); 'Underwater Conservation' (G. Wever); 'Sea-level changes in Britain' (A. Ackeroyd), and a one-day conference on underwater surveying.

In 1968 a course of five lectures was organised under the joint auspices of the CNA and the Institute on 'Sources of Maritime Archaeology', and the URG entertained the Mensura diving team who showed film of their work in Tharros, Sardinia. Paul Johnstone showed film of George Bass's expedition to Yassi Ada, Turkey, and H. Wylde joined Dr Katzev to work on the Kyrenia wreck. Following years saw lectures by S. Swiny on underwater archaeological techniques, R. Piercy on a wreck in Cyprus, and Honor Frost on work in the Mediterranean, as well as public lectures by her in association with the Palestine Exploration Fund and CNA.



Plate 3 Dr John Waechter (right) demonstrating drawing techniques to the marine archaeology class.
Photo: *The Times* (31.5.1965).

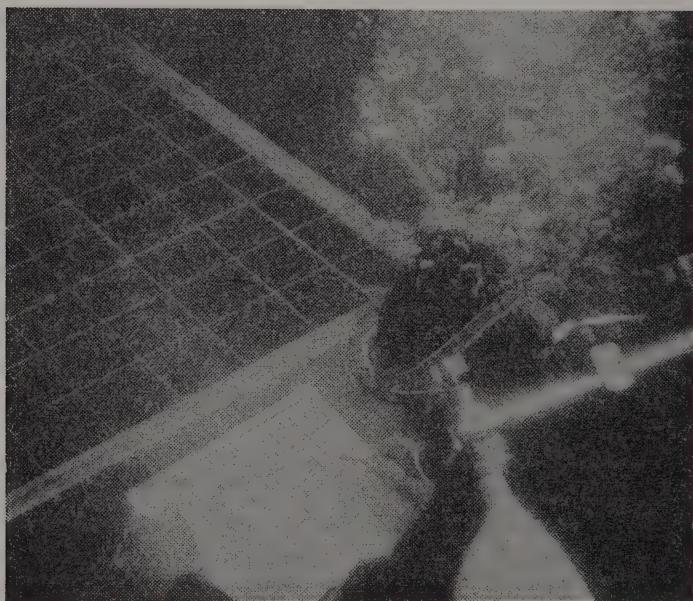


Plate 4 In to practice: scale drawings being prepared on the mock wreck (1965) Photo: *Sunday Telegraph* (4.7.1965).



Plate 5 The archaeologists' friend: drawing frame at Swanage (1965) with team (photograph courtesy of M. Varese).

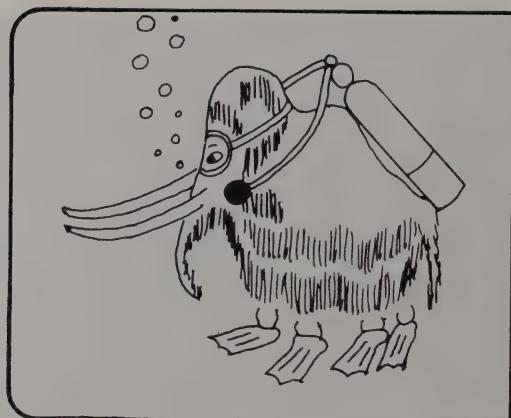


Plate 6 The Underwater Research Group's logo!

The URG fell into abeyance from 1970 until 1975 when two Institute undergraduates, Martin Dean and Chris Preece reformed the group (under stimulus from Joan Taylor and John Waechter) and re-established a programme of diver training for Institute students (Plate 6). With a healthy membership, it was within six months invited to conduct a survey of a suspected early 16th century wreck off Plymouth by the Cattewater Wreck Committee (under the Chairmanship of Lt.-Commander Alan Bax). The wreck had been discovered in 1973 as a result of dredging in the River Plym, and she became the first site to receive emergency designation under the Protection of Wrecks Act 1973. The timbers then examined suggested that further investigations would provide highly significant data for the study of this crucial period of ship design. Three seasons' of work on the site with the support of Fort Bovisand Underwater Centre and Earthwatch (Plate 7) resulted in the first full publication of an underwater wreck in British waters (Redknap, 1984). I undertook the running of the URG in 1977 and it organised a successful exhibition during that academic year entitled 'Archaeology Underwater: a case of mistaken identity'. Further URG projects were to follow.

In 1981 the URG was approached by Nikos Efstratiou (then Institute postgraduate under Professor Evans) to conduct a survey of a submerged Neolithic site on the island of Aghios Petros (Northern Sporades, North Aegean). Cathy Giangrande, newly appointed President of the URG contacted Dr N. C. Flemming from the Institute of Oceanographic Sciences, and under his guidance she put together a team of ten divers, archaeologists and photographers from the URG, Cambridge University's Underwater Exploration Group, the Institute of Oceanographic Sciences and the Greek Archaeological Eforiate of Volos. They spent six weeks on the uninhabited island surveying the submerged portion of the site. Excavations on land had uncovered Neolithic material with an abrupt break in the distribution at the edge of the island, suggesting that the site may originally have extended into the sea. During the underwater survey, several one metre square frames were placed on the bottom



Plate 7 Checking diver's log during final season of work on the Cattewater wreck (1978).
Photo: E. Brown.

and one was excavated to produce 347 Bronze Age and Neolithic sherds, 123 pieces of goat and sheep bone, shell and worked obsidian/flint. The results of further test pits were similar, at depths ranging from 3.8 to 8.8 m below present sea level, implying the existence of a coastal settlement with trade connections with the mainlands of both Greece and Turkey (Flemming, 1983; Flemming, 1985, 22; C. Giangrande, pers. comm.). Evidence of submerged solution notches indicated recent land submergence of 30–50 cm below mean sea level.

This was followed in 1982 by an invitation from Dr Vassos Karageorghis (Director of the Department of Antiquities, Cyprus) to the URG to survey 13 km of coastline from the Late Bronze Age site at Maa (just north of Paphos) to the Lara peninsula, to locate archaeological sites and relate this evidence to the changing geomorphology of the area. The 1983 team comprised divers, archaeologists and geologists from the URG, Cambridge University Underwater Exploration Group and the Geology Department, University College London. Under the archaeological direction of Jon Adams (1983) it surveyed two large bays surrounding the Maa peninsula (Fig. 1). Subsequently search areas were carefully selected and sherds of Cypriot pithoid amphora (Archaic period) identified. On a more exposed stretch of coast to the north of Kerati, several mounds of amphora sherds were located at a depth of c. 3 m. A total of eight sites were examined and in most cases a considerable quantity of material recovered. The survey, as a systematic study of shallow water sites over a large area of the coast, showed that even such exposed underwater sites yield data which, when integrated with geomorphological evidence assists the reconstruction of coastal activities (Giangrande,



Fig. 1 Distribution of potsherds plotted in Keratidhi Bay (Cyprus Underwater Survey 1983).

Richards and Adams forthcoming). The URG continues to organise training, lectures and projects for the continuing student demand.

The formation of the Nautical Archaeology Society sponsored by CNA and NAT marked a new era in the development of nautical archaeology in Britain. At the inaugural meeting on 27 May 1981 at the Institute, Joan du Plat Taylor agreed to become its first president. The idea of a membership society to spread the cause of nautical archaeology at all levels and to improve standards of recording, excavation and conservation was first proposed by Professor Grimes in 1974 (as Chairman of CNA). Professor Grimes retired shortly after, and it took five years to convince doubters that an independent subscription society could survive without being taken over by treasure hunters. Happily membership has grown to nearly 500 and the NAS continues to hold its AGM at the Institute. Following Joan's death in 1983, the NAS Executive Committee inaugurated an award to be made periodically to any person in the UK or abroad for outstanding contribution to the subject, known as the Joan du Plat Taylor Medal.

While the Protection of Wrecks Act provided a much needed method of site protection, shortcomings in the structure of nautical archaeology in Britain were highlighted by events in 1979/80. In March 1979 while working on postgraduate research at the Institute, I received a phone call from Mr F. Wall, a member of the Isle of Thanet Archaeological Unit who had diligently been advising a group of local divers (who had started to examine wrecks off

Ramsgate) on archaeological methodology and the need for recording. 'One wooden wreck is standing over 20 feet off the bottom. . . would you be interested in inspecting the site, and giving the lads some lectures and advice?' Arrangements were immediately made. Subsequent inspection of the site by Martin Dean, the late Keith Muckelroy, David Lyon (naval historian at the National Maritime Museum), myself and several other visitors established the veracity of Mr Wall's initial description: the divers had stumbled upon the well preserved wreck of the *Stirling Castle*, one of four 70-gun line-of-battle ships to be wrecked on the Goodwins during the 'Great Gale' of 1703 (Lyon, 1980, 339). The sands had shifted, exposing her vast semi-coherent structure and exposing her to degradations of irresponsible divers as well as marine organisms. Even the Protection of Wrecks Act, under which she became designated, could not prevent her eventually falling apart, and no existing group had the resources to undertake its complete archaeological recording. License to survey was granted to a local team under the Isle of Thanet Archaeological Unit, but their resources were dwarfed by the immense task involved. This was not all. Two further wrecks from the same Great Gale of 1703 had been exposed and located, spotlighting the infamous 'Ship Swallower' as an important but poorly understood warehouse of archaeological knowledge. In December 1982 a meeting was arranged at the Institute of concerned archaeologists, historians and specialists in the fields of remote sensing and offshore technology to discuss the Goodwins problem and see whether modern technology could assist in identifying archaeological sites in this maritime zone, establish their distribution and assess their states preservation. This resulted in the formation of the Goodwins Archaeological Survey, which soon developed into the registered charity MAS (the Marine Archaeological Survey).

Some twenty-two years ago Joan Taylor stressed a need for a record of the information collected by divers to be collated in some central national record. ' . . Only then can the implications as regards trade routes and anchorages and so forth be clearly understood in relation to the general archaeological background' (Taylor, 1964, 190). MAS has undertaken geophysical surveys of areas of the Goodwin Sands (1983), Copperas Channel (1985), and Margate Sand (1986: Fig. 2), and assisted other maritime projects by providing geophysical data—in 1986 on a 17th century wreck in Yarmouth Roads (Isle of Wight), and by searching for a postulated 10th–12th century vessel off Southwold (Suffolk) for the National Maritime Museum (Hutchinson, 1986, 219).

As official locus for the project, and regular meeting place for its committee, the Institute has played an important role in launching the Marine Archaeological Survey, which continues to work towards a regional marine site register for the south-east coast (Redknap and Flemming, 1985; Redknap and Emptage, 1986). The proposed extension of British territorial waters out to twelve miles substantially increases the scope of nautical archaeology round Britain. The limitations of the three mile limit were dramatically and tragically illustrated in 1985 by the fate of the wreck of the East Indiaman *Admiral Gardner* which went down on the Goodwins in 1809. The use of modern salvage techniques such as prop washes on the site resulted in an application for designation under the Protection of Wrecks Act which failed when divers established that the site lay just outside current (1985) territorial waters: this left the field free for 'goodie hunters'. The same year saw the destruction by mechanical grab of the

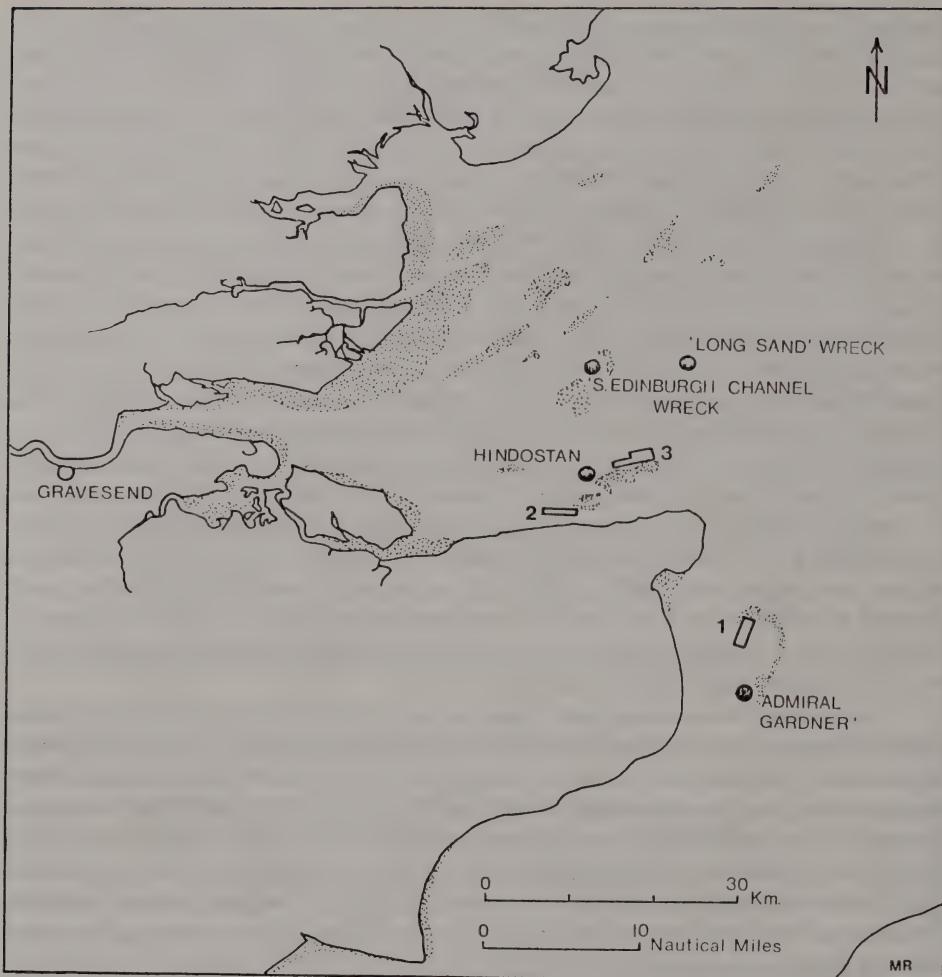


Fig. 2 Surveys by M.A.S. in south-east Britain. 1=Goodwins 1983; 2=Copperas Channel 1985; 3=Margate Sand 1986.

wreck of the outward bound East Indiaman *Hindostan* (1803) in the Thames Estuary, and of the wreck of the outward bound East Indiaman *Albion* wrecked off Long Sand (1765), lying just outside territorial waters. Valerie Fenwick, CNA chairman, took steps to ensure that historic wreck protection was specifically included in the provisions of the 1987 Territorial Sea Bill. In moving the key Second Reading, the Minister responsible, Baroness Young said: 'I fully understand the concern of nautical historians and archaeologists. I confirm that the effect

of the Bill is that the Protection of Wrecks Act 1973 would automatically apply in the wider area of the territorial sea'.

In 1979 the Department of Trade produced a 14-page illustrated booklet listing the 19 wrecks then designated. The December 1986 revision listed 32 wrecks. Sometime earlier the Minister, John Clinton Davies, had visited and dived on the *Mary Rose*, but later Ministers have shown little interest. A questionnaire circulated by the CNA Law Sub-Committee for the 1983 anniversary showed a number of criticisms of the Act's administration – from insufficient archaeological representation on the advisory committee to complaints on enforcement. Some of these shortcomings may be rectified by the recently established Archaeological Diving Unit based at the University of St Andrews under the able leadership of Martin Dean. Further advances may be made through the application of the Ancient Monuments and Archaeological Areas Act 1979, with its as yet dormant provisions for monuments in territorial waters. It was a landmark for nautical archaeology when it reached the statute books for it recognised the indivisibility of the field, whether above or below water, by extending the possibility of ancient monument status and protection to wrecks and structures within territorial waters, thanks to CNA intervention during the final report stage of its passage through the Lords in February 1979.

Advances may also be made by the Institute of Field Archaeologists work towards defining ethical and professional standards in underwater archaeology through its newly formed Maritime Affairs Group (March 1987). An alarming development of recent years has been the growth of enterprises intent on treating marine archaeological sites as commercially exploitable resources – both abroad and in British waters.

The massive Law of the Sea conferences during the mid- to late- 1970s which lasted many months and were mostly concerned with mineral rights in international waters, formed one platform to obtain legal recognition of our underwater heritage at international level, though a forlorn one for nautical archaeology questions. In contrast, the Council of Europe took on 'the underwater cultural heritage' as something of a special project and a great deal of excellent work was done – and published – comparing the law of different countries on wreck, salvage, cultural protection, and so on. CNA's politician David Blackman, now working in Brussels, had much to do with this and the report produced by John Roper MP in 1978. CNA members were able to put the rapporteur in touch with the international lawyers Lyndel Prott and Patrick O'Keefe of Sydney University who acted as legal advisers. They have now produced the definitive work in this field (O'Keefe and Prott, 1984). The convention binding European nations to respect the underwater heritage is not yet open for signature despite adoption of the Roper Report recommendations. These included calls to revise existing legislation to plug loopholes in protection and to extend this Act to the 200 mile limit all round Europe.

The threats to nautical archaeology are not new: John Waechter, on opening his school at Swanage in 1965, complained of divers stripping wrecks. While the wind blows and the waters remain choppy, while our finite underwater heritage remains under threat of salvage hidden at times behind a thin archaeological veneer, the Institute must keep a beacon fire burning.

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Archaeological Conservation: The Development of a Discipline

by NIGEL J. SEELEY

Introduction

Sir Mortimer Wheeler's often quoted intention that the Institute of Archaeology should be a 'laboratory which shall fulfil in the study of civilisation something of the function which the laboratory has long fulfilled in the study of chemical or physical science', emphasised the application of scientific disciplines to archaeology from the outset. The different aspects were, however, given different roles, and while, for example, environmental archaeology was seen from the start as a discipline in its own right, conservation started out as a technical support facility within field archaeology and was essentially taught as a practical technique. The purpose of this lecture is not solely to illustrate the Institute's involvement in the development of conservation, but to try to show something of what the subject is, and how it came into existence.

I shall limit what I am saying almost exclusively to archaeological conservation, in other words the conservation of all classes of material from archaeological excavations, including stone, ceramics, glass, metals, and organic materials of both plant and animal origin from both wet and dry environments. For practical purposes it is often convenient to include ethnographic artefacts and also some historical objects in this category, as they usually require the application of similar techniques, and are often found in one and the same museum. While excavated wall-paintings certainly come within this category, paintings of a non-archaeological nature are specifically excluded, forming as they do a field of conservation in their own right. Such topics as the conservation of library and archival materials, and the conservation of natural history or geological specimens have their own history, and have also become separate specialisations. The almost completely unrelated subject of environmental conservation is often confused with its archaeological namesake, even though its practitioners generally describe themselves as 'conservationists', as opposed to the more object-orientated term 'conservator'. This seems to have come into the English language from the German and does not correspond in meaning with the more curatorial implication of the French word 'conservateur'.

In discussing the history of a subject, it is customary to quote early references which may illustrate aspects of its development. As one might expect, there are few of any great age

relating to conservation, but I remember having had my attention drawn some years ago to a passage in the Book of Ecclesiasticus, Chapter 22, Verse 7:

‘Whoso teacheth a fool, is as one that gleweth a potsherd together.’

Perhaps I should have taken this as a warning, but I hope to show tonight, if nothing else, that there is more to archaeological conservation than the sticking of pots.

History

The distinction between conservation and repair did not of course emerge until long after the commencement of serious study of the remains of the past. In the ancient world, works of art from an earlier period were of course restored and copied, but such repairs as were made were carried out by craftsmen in the materials concerned, with probably no other aim than to correct any damage which had been suffered by the object.

The deterioration of valuable objects was, however, frequently commented on, and in many early cultures strenuous efforts were made to protect vulnerable and important classes of material, especially the written word, either by the use of protective substances resistant to damp, fire, or insect attack, or by providing an enclosed and controlled environment. Pliny, to whose *Natural History* one can usually turn for information of varying degrees of accuracy on the technical ideas of the Roman world, gives several examples, including the use of cedrium or cedar-wood oil for the protection of papyrus from insect attack. He also mentions the discovery of well-preserved papyrus books in a 535-year-old tomb, which he ascribed to the presence of citrus leaves found with them having provided protection against deterioration. There are a number of other instances of exceptional preservation of items from the past in ancient and medieval literature.

That conservation was essentially the province of the craftsman, and not a serious consideration in the study of artefacts, is apparent from its lack of mention in most early writings on art and antiquities. That is not to say that no interest was taken in the materials from which antiquities were made, or even in the aesthetic appreciation of the consequences of their deterioration. The Chinese treatise on connoisseurship known as the *Ko Ku Yao Lun*, or the *Essential Criteria of Antiquities*, which was first printed in China in 1388 and translated into English and edited by Sir Percival David (1971), gives valuable information on the nature and sources of the raw materials from which many different types of art object were made, and describes the ways in which they could be identified. It also discusses at some length the corrosion phenomena found on the most ancient Chinese bronzes, which were so highly prized during the Sung and Ming Dynasties. It even talks of the forgery of these antiquities, and the ever-present problem of false patination, but the only mention of any aspect of conservation which I have been able to discover in the work is a warning against the too-frequent re-mounting of scroll paintings and the damage which this could cause.

The repair of antiquities as a means of prolonging their useful life must go back to remote times. Such items as have reached us from the European Middle Ages which could be described as having been conserved are almost invariably small and precious relics of antiquity

which have been repaired and re-mounted to give them a new lease of life, and have achieved the status of curios. Examples include the tenth-century Lothair cross, with an ancient cameo of Augustus repaired and set in it, and a twelfth-century chalice made from the remains of a Roman sardonyx cup. Because it was indeed the very craft-basis of the subject which led to the lack of early written records, it is especially important that one of the earliest references to conservation as we understand it today comes from the pen of none less than Benvenuto Cellini, the Florentine goldsmith and sculptor. In his memoirs (1847), he describes a hoard of small bronzes found in the district of Arezzo in about the year 1558, which came into the possession of Duke Cosmo of Florence. These figures were encrusted, he says, 'either in earth or rust', and many had parts such as arms or heads missing. Cellini describes the mechanical cleaning of these bronzes by himself and the Duke using goldsmiths' chisels, followed by the sculpting and replacement of the missing parts, and emphasises the considerable amount of time such work involved. It would be interesting to know whether any of these bronzes can still be identified, as they would be of great interest from both the conservation and art historical point of view.

This passage is significant not only because of its early date, but because it also illustrates a controversial aspect of the conservation of excavated metal artefacts. With the exception of items of gold, these are almost invariably corroded and, in the process of corrosion, some or all of the surface detail will have become converted into corrosion products. In many instances, and particularly in the case of bronzes, careful cleaning by mechanical means will reveal the detail preserved within the corrosion layers. This, however, requires great skill and the expenditure of much time, but it is highly controllable and often reveals the sharpest possible detail on an aesthetically pleasing patinated surface. The alternative approach is to use chemical methods. These should not damage the metal if applied correctly, but will remove much if not all of the corrosion products and will usually result in significant loss of detail. The surface looks metallic, is often pitted or etched and lacking in sharpness, and not pleasing in appearance. In their favour, chemical methods are relatively rapid and require little manual skill. The development of conservation in a scientific context led, not surprisingly, to the widespread adoption of chemical techniques, and it is really only over the last two decades that there has been an appreciable return to mechanical methods, sometimes assisted by carefully controlled and highly selective chemical cleaning.

Other tantalisingly brief glimpses of conservation appear from time to time. The Napoleonic Commission accompanying the French Expedition to Egypt in 1798, and well known for its acquisitive activities, was composed of 167 scientists and technicians from various fields of endeavour. One of these, the mineralogist and vulcanologist Deodat Gratet de Dolomieu, became a specialist in the selection, conservation, and transportation of ancient monuments.

It appears that the first serious attempt at what we would recognise as an archaeological conservation project arose from the discovery of some 1,696 carbonised papyrus rolls in 1752 at Herculaneum, in the ruins of a villa which has since become known as the Villa of the Papyri. These had been buried under a considerable depth of volcanic ash and had become darkened and highly embrittled. Fragments removed showed them to contain Greek and a few

Latin texts and there was considerable expectation of the discovery of lost works. A method for unrolling these papyri was developed in the late eighteenth century by one Padre Piaggi, a sub-librarian in the Vatican, which involved the step-by-step attachment of the unwritten outside surface of the roll to a thin skin with animal glue, followed by careful unrolling of the mounted section. By supporting the roll on a frame, alternate glueing and lifting operations were often successful in separating long passages of text, which were then immediately copied by professional scribes. The Prince Regent not only acquired copies of 94 of these rolls for the Bodleian Library, Oxford, but sent his chaplain John Hayter to assist in the work of unrolling. Hayter had to leave Naples before completing the work because of the developing political situation, but his account (Hayter, 1811), contains what are probably still the best botanical illustrations of the papyrus plant of any period.

We learn that under Hayter's supervision, some 1,000 columns of different works were unrolled, but it had rapidly become apparent that the success rate was very low and was limited to those papyri which had suffered the least deterioration. Continuing interest by the Prince Regent, by now King George IV, led in 1818 to a major development in the approach to conservation. The King instructed Sir Humphrey Davy, President of the Royal Society and Director of the Royal Institution – the most influential scientist of his day – to look into the opening of the remaining Herculaneum rolls. His report, published in the *Philosophical Transactions of the Royal Society* for 1821, shows clearly that he anticipated a number of important developments in conservation which still hold firm today. These included the analysis of the papyri themselves, in order to discover the nature of the deterioration and hence to predict the best approach for conservation; the use of chemical reagents, in this case chlorine, to remove the natural adhesive substances which were one of the principal obstacles to unrolling them, and the formulation of an adhesive with exactly the properties required for the task in hand. The further observation that apparently charred organic materials have not necessarily been exposed to the agency of fire, is one which is still not infrequently overlooked by archaeologists. Nor was this Davy's only excursion into the past. Several years earlier he had investigated the pigments on classical wall-paintings and vases, and had related his findings to information in the works of Pliny, Theophrastus, and Vitruvius.

Davy's involvement with the Herculaneum papyri marks the beginning of the second stage in the development of conservation, namely the intervention of able scientists in various fields who do not otherwise have any professional contact with the world of archaeology. This was the situation which prevailed through much of the nineteenth century, and into which some of the best scientific minds of the time were drawn, although it is true to say that much more effort went into investigations aimed at discovering the composition and method of manufacture of artefacts than into the conservation processes themselves.

To Dr John Davy, Sir Humphrey's less well-known brother, goes the distinction of having been the first person to investigate the mechanism of corrosion of metallic antiquities. As early as 1826 (Davy, 1826) he published the results of his findings on the examination of a Greek bronze helmet, and not only identified correctly four of the compounds we most commonly find today, but placed the process in the context of the electrochemical theories recently expounded by his brother.

Sir David Brewster, unusually amongst physicists, also took an interest in the deterioration of archaeological materials. It was he who first explained the iridescence of certain examples of ancient glass in terms of the microscopically thin onion skin layers forming on the surface, relating this to the interference of light passing through thin transparent films such as soap bubbles (Brewster, 1861).

This work, however, led on to an extraordinary paper 'On the process of decay in glass' by James Fowler, FSA, which appeared in *Archaeologia* for 1880. Fowler was an antiquarian and seems to have written no other scientific papers but, in a space of 98 pages, he not only examines the history of glassmaking in great detail; classifies the different manifestations of its deterioration with considerable scientific competence, and attempts to relate these phenomena to the source of the glass and also to its composition. He contrasts the behaviour of glass with that of metals, whose deterioration seemed to him to be much less dependent on environment, and clearly recognises such phenomena as weeping glass, crizzling and attack by fungal organisms. Further, he relates some of the deterioration in stained glass to the detrimental effects of gas lighting, thus anticipating the fight to protect antiquities against the ravages of atmospheric pollution, which has really only been taken seriously during the last 20 years or so.

Official involvement in conservation in the mid-nineteenth century was almost invariably directed towards specific urgent problems, for example the rapid stone decay observed in urban environments, the deterioration of wood carvings in the South Kensington Museum, and the lack of permanence of paper and bookbinding leather. Much of the early work on stone conservation was of an empirical nature and originated in the context of architectural conservation, becoming assimilated into museum practice by the way. Not infrequently, early attempts at conservation did more harm than good through a lack of understanding of the principles involved; Sir Giles Gilbert Scott's experiments with the fabric of Westminster Abbey in the 1850s led to many problems for his successors.

We know very little of the minor figures associated with conservation before about the 1870s. Henry Layard, in the context of his Nineveh excavations, praises one Mr Doubleday of the British Museum for his great skills in conserving the small finds, in particular the ivories, although he of course says nothing about techniques, and of Doubleday himself, little more information can be gleaned. Heinrich Schliemann, probably like other archaeologists of his day, appears to have done much of his own conservation, and talks of restoring the pottery from Troy with shellac and plaster, although he called for the assistance of a druggist when it was found necessary to conserve a rather well-preserved body from one of the tombs at Mycenae. Almost alone among the archaeologists, Flinders Petrie took conservation very seriously, and not only wrote a paper on 'The treatment of small antiquities' in 1888, but in his later book *Methods and Aims in Archaeology* (1904) he devoted a whole chapter to the subject, based on his personal experience as an excavator. The conservation efforts of Sir Arthur Evans at Knossos are too well known to require repeating, but illustrate dramatically the need to guard against any restoration which passes beyond the certain proof of the archaeological evidence.

The next major breakthrough in the development of the subject was the setting up of

formal conservation facilities within major museums and the employment, in some cases, of scientifically trained assistants to staff them. The Royal Berlin Museums were among the earliest to do this and much empirical conservation experience was amassed in this way.

From this background emerged an extremely significant book, the *Merkbuch, Alterthümer aufzugraben und aufzubewahren*, a pocket guide to the excavation and preservation of antiquities. This work, which was of the nature of an official publication, appeared in Berlin in 1888, and went through two further editions in 1894 and 1914, but appears to be virtually unknown – I have been quite unable to trace a copy of the first edition. Contemporary evidence suggests that the author was Dr Albert Voss, Director of the Berlin Museums, and that the section on conservation was either by, or drawn from the writings of, Eduard Krause of the Museen für Volkerkunde, Berlin. Krause was the most active of the early conservators in Berlin, but it is now rather difficult to distinguish his personal contributions to the subject from those he borrowed from other sources. Whether or not part of the *Merkbuch* was actually written by Krause, its importance is that it represents the first attempt to provide a textbook of conservation methods along scientific lines, with a wide coverage of inorganic and organic materials. It may also, possibly, repay study in the context of the early history of field archaeology.

One reason for the probable obscurity of the *Merkbuch*, is that it was superseded to all intents and purposes in 1898 on the appearance of a much more comprehensive work on the same subject, Dr Friedrich Rathgen's *Die Konservierung von Altertumsfunden*. Rathgen provides the earliest instance of a scientist being employed in a dedicated conservation post in the museum world, becoming the first Director of the Chemical Laboratory of the Royal Museums of Berlin in 1888. Born in 1862, Rathgen studied chemistry at Göttingen, Berlin, and Marburg, obtaining his doctorate in 1886. During the 39 years of his Directorship, he made original contributions to aspects of the conservation of almost all archaeological materials, although his approach to the work was perhaps predictable for a chemist with little or no background in archaeology or early technology. Like a dental surgeon, his aim was to vigorously eliminate decay wherever and whenever he found it, and to remove traces of any substances which might lead to further deterioration. His 60 or more publications included aspects of the conservation of many archaeological materials, and his book appeared in an English translation in 1905, hardly diminishing in influence until the appearance of Plenderleith's work in 1956.

Rathgen's personal contributions to the subject are too numerous to mention in detail, but it was he who developed the process of firing as a means of preserving the unbaked clay tablets of the Middle East which provide the source of most of our historical information on these civilisations. These often could hardly be handled when found, rapidly disintegrating in museums through the agency of the soluble salts with which they were almost invariably loaded. While firing is most definitely not a reversible process, it is nevertheless extremely effective, and has never been superseded for this class of object.

To Rathgen, also, we owe the introduction of synthetic materials into conservation. The traditional adhesives, consolidants and surface coatings were all natural substances such as starch and animal glue, resin varnishes, and shellac. These were rarely colourless, deterior-

ated unpredictably, and could frequently not be removed when required. Among the first of the artificial resins was nitrocellulose. This was developed for military purposes under the name guncotton and might not automatically come to mind in a search for a preservative material for use in conservation. Dissolved in a suitable solvent, however, it forms an excellent adhesive and will also provide a thin and colourless protective coating on objects. Its large scale use as a film base was long ago discontinued because of the considerable hazards involved in storing or handling the material in bulk, but it is still used as an adhesive in conservation, despite other known disadvantages, largely for the lack of commercial incentive to produce a substitute which still retains its most important properties. Rathgen employed a commercial nitrocellulose marketed under the trade name of 'Zapon', and at a later date also advocated the use of 'Zellon', a variety of the much less inflammable cellulose acetate now used for safety film bases. Another area pioneered by Rathgen was the recognition that conservation does not end with the treatment of an artefact. Rathgen carried the concept of stabilisation into the museum and reconciled the requirements of storage and display with the need to provide a suitable environment for an antiquity. It was he who advocated the preservation, and indeed display, of unstable artefacts in a suitable liquid, a successful if cumbersome measure which has only really become obsolete since the advent of such techniques as freeze-drying.

From what has already been said, one might be forgiven for thinking that archaeological conservation had been almost wholly a preserve of the German museum establishment and, indeed, there is an element of truth in this. Developments were, however, also taking place elsewhere. The eminent French chemist and historian of chemistry, Marcellin Berthelot (1827–1907), became deeply interested in the origins of metallurgy through his work on alchemy and on early technological texts. His contribution to conservation, while valuable, was limited to investigations into the mechanism of corrosion of ancient metallic artefacts. More extensive contributions were made by a Greek chemist, Otto Rhoussopoulos (1855–1922), who had studied in Berlin and had no doubt seen something of the work in the museums there. He became involved in conservation on his return home and quickly changed his publishing language from Greek to the more acceptable German in order to reach a wider potential readership.

Not surprisingly, the first conservation laboratory established in the United Kingdom was that of the British Museum. This was not initially intended to be permanent, but was set up after the First World War to rectify damage resulting from wartime storage in unfavourable conditions such as railway tunnels. The Department of Scientific and Industrial Research appointed Dr Alexander Scott, a distinguished chemist and Fellow of the Royal Society, to this post. In the event, Scott stayed for over 20 years, retiring in 1938, having firmly established conservation at the Museum. His work at the Museum is documented in his three classic reports, in which his concern for metallic antiquities manifests itself. This in part reflects the urgent need for conservation of these materials, which may become highly unstable under unfavourable conditions and corrode rapidly. It seems also to have been an aspect of conservation particularly favoured by the early workers because of the much better understanding of the deterioration processes in metals as opposed to those of many other materials. Scott was involved on a number of occasions with the conservation of objects outside the British

Museum, for example the funeral achievements of the Black Prince in Canterbury Cathedral, and in his assistance with some of Carter's finds from the tomb of Tutankhamen.

Another Englishman to become deeply involved in archaeological conservation was Alfred Lucas. Born in 1867, and trained as a chemist at the Royal College of Science, now Imperial College, Lucas went into the Laboratory of the Government Chemist. During this period he also lectured in Chemistry at Birkbeck College, but ill health forced him to take up an appointment in Egypt, where he spent the remainder of his life, setting up the Egyptian Government Chemical Department and becoming its first Director. Lucas is best known for his pioneering work *Ancient Egyptian Materials* (1926) in which he paved the way for the study of early Egyptian technology. He was also, however, widely involved in other aspects of Egyptian studies including conservation, and another of his books on *Antiquities: Their Restoration and Preservation* was published in 1924 and revised eight years later. Lucas' most important single conservation project was his major involvement in the conservation of the finds from the tomb of Tutankhamen, Howard Carter having obtained his services from the Government for this purpose.

By the mid 1920s, conservation was firmly established as an essential service in the major museums of the world. In New York, collaboration between the Metropolitan Museum and the University of Columbia led to refinements in the treatment of ancient bronzes (Fink and Eldridge, 1925), and similar trials were carried out at the Field Museum in Chicago (Nichols, 1930). These emphasised the chemical or electrochemical techniques, which have already been contrasted with more controlled methods.

Although conservation was by now becoming well established in the museum context, there was still no systematic provision for it in archaeological fieldwork; few, if any, excavations ever saw a conservator in the flesh. It is noteworthy that even the great innovators of excavation technique such as General Pitt Rivers paid little attention to the potential for the scientific study of the artefacts he recovered. It was left to the Archaeological Survey of India to lead the way in this as in other fields. With few constraints and a vast number of sites and monuments requiring investigation and protection, the Survey was free to introduce new ideas and to employ them throughout the sub-continent. As part of Lord Curzon's re-vitalisation, Sir John Marshall was appointed Director General of Archaeology in 1902. Curzon took an active interest in the conservation of standing monuments from the Parthenon to the Taj Mahal, and personally restored Bodiam Castle in Sussex. In Marshall, he found a man who shared his appreciation of the need for conservation, not only of the monuments themselves, but also of the artefact material. Marshall's initiative in this respect was shown early in his pamphlet on *The Conservation of Ancient Monuments*, published in 1907, followed by his *Conservation Manual* in 1923. The latter, which is still in use in India, deals largely with standing monuments, but in recognition of the need for artefact conservation, Marshall succeeded in establishing a post of Archaeological Chemist to the Survey in 1913, for the purpose of 'cleaning and preservation of antiquities'. Wishing to appoint an Indian to this post, and having no qualified candidates, Marshall selected one Mohammed Sana Ullah, a chemist working in the Indian Ordnance Department at Naini Tal, and sent him to England for training. According to a later account by Ullah himself (1939), he received instruction at the

British Museum and at University College London. This latter statement was so interesting that I turned to the College Records Office for further information and learned that Ullah was indeed admitted as a postgraduate student in chemistry in the academic year 1914–15, but does not appear to have completed a course of research or to have been awarded a degree. Returning to India in June 1917, he was based temporarily in the Indian Museum in Calcutta, but began the development of conservation facilities for the work of the Archaeological Survey, setting up field laboratories at Mohenjo-Daro, Harappa, and Taxila. He counted the scientific investigation of antiquities within his remit; hence Marshall's comprehensive reports on his excavations at Mohenjo-Daro and Taxila are among the first to include a substantial body of scientific analyses. Ullah's original contributions to the techniques of archaeological conservation are few, but his extension of laboratory techniques to the field was a notable step forwards. Although retired, he contributed a paper on his work to the first issue of the journal *Ancient India*, founded by Wheeler in 1946 when he was himself Director General of Archaeology for India. In the context of the Institute's Jubilee celebrations and the merger with University College, it was a pleasant surprise to discover that the holder of the first ever official archaeological conservation post can be counted, loosely at least, among our former students.

During the last 60 years, an increasing number of museums has been provided with conservation facilities, and conservators have begun to become equated with the curatorial staff in terms of career structure, if not always quite without friction. Similarly, archaeological services and units worldwide have recognised the need for conservators in the field, and in some countries legislation is making the employment of a trained conservator a prerequisite for the issue of an excavation permit.

The subject

If conservation is to fully justify itself as a serious discipline, it must certainly do more than concern itself with the restoration of artefacts for exhibition purposes and, fortunately, the subject has considerably more potential than this. In the first place, it is the conservator who is normally in the best position to examine closely the material from an excavation. Even with fairly basic equipment and facilities, he or she can often distinguish features not immediately visible to the archaeologist and draw conclusions as to the materials and techniques used in their manufacture. In the museum context, the conservator may detect earlier restoration, or even outright forgery, or may be able to give an opinion on provenance on the basis of technology. By placing on the conservator the triple obligation of investigation and recording as well as preservation, a real contribution to the research element of an excavation can be made.

Conservation need not always be seen in the light of active intervention. Much money and valuable resources are wasted today by over-conservation, whereby minor finds are treated at sometimes considerable cost, only to be drawn or looked at by the archaeologist and then placed in store where, more often than not, they deteriorate beyond recall. It is felt, rightly, that the privilege of excavating confers the onus of making the maximum possible use

of the material recovered. This can, however, be done in a number of different ways. If all finds are examined by the excavator and by an experienced conservator, it will usually be possible to select those of archaeological, art-historical, or technological importance, and these may then be given detailed treatment. For the others, full recording and then storage under favourable environmental conditions may well be more beneficial in the long term, and certainly more economical, than indiscriminate attack by a conservator. The very bulk of the material held by museums and archaeological units makes passive conservation of great importance and also demonstrates the need for better resources.

It is a commonly held claim among conservators that the treatment of an object should be, if not non-interventional, at least without invoking permanent change in any aspect of the artefact. In reality, this is rarely achievable, even in the case of processes which are nominally reversible. The more the materials of antiquity are studied, the more frequently it is found that not only chemical agencies, but also light, warmth, humidity, X-rays, or mechanical stress can destroy evidence inherent in the artefacts. Not only may features disappear, but modern contamination may cause the addition of substances not originally present in the object. This, alone, would make obligatory the fullest possible examination and recording of an object both before and during the process of conservation, in order to preserve information which might otherwise be lost for ever. The essential stages in the conservation process must therefore be investigation, recording and preservation, in that order.

Reconstructive conservation, whether it be termed repair or restoration, almost invariably requires the addition of new material to an artefact. There have been in the past, and indeed still are, two approaches to this problem: the so-called traditional repair, matching like with like, and the use of modern synthetic materials. While traditional materials do not always have precisely the required properties, they are, by their very nature, predictable in their ageing properties and in countries like China and Japan, are widely used in conservation. The alternative, namely the use of modern synthetic substitutes, can provide materials with precisely the properties required, but their long-term performance may not be reliably known. Whatever the materials or techniques employed, the archaeological integrity of an artefact must at all costs be retained and reconstructions and additions carried out only if there is certain evidence of their original form. No attempt must be made to falsify, or to conceal the difference between original components of an artefact and any restorations.

There is a more than coincidental analogy between conservation and medicine, which goes some way towards explaining the emerging structure of the profession: the relationship between the practitioners at the bench and those engaged in administration or research. Each discipline has diagnostic, preventative and clinical elements; each is dealing with a clientele which, while falling into broad but recognisable categories, exhibits a considerable degree of individuality. The conservator has, therefore, to have a foot in many camps. He needs to understand something of the civilisations of the past, the history of technology and the sources and nature of the raw materials used for the manufacture of artefacts. He has also, however, to be able to use scientific techniques to identify the artefacts he handles, understand their deterioration, and propose and carry out procedures for their treatment, often requiring the use of delicate manual skills. In addition, he is often expected to come up with an answer to a

situation which he has not previously met. Clearly few people can satisfy all these requirements, although such has often been expected of conservators in the past.

There is also the rather intangible faculty namely the 'feel' for antiquities, without which conservation becomes little more than a mechanical process. This, like a good bedside manner, is something which cannot be taught, it usually develops with experience, and some individuals completely fail to acquire it. Allied to this is the quality of patience, which is essential in all fields of conservation, and which probably accounts for the fact that at least 75% of conservation students passing through the Institute are women, in some years the proportion is even much higher than this.

With a subject of such breadth, the need for specialists in particular materials has often been raised. Indeed, a few of the larger museums actually sub-divide their conservation departments into materials specialisms. While this may work well in some instances, only the largest organisations have sufficient staff to allow such an arrangement, and it may mitigate against such specialist conservators finding employment in the smaller museums where a wide range of skills may be needed. For these reasons it has seemed preferable to keep the Institute's course broad in scope and thus train the general practitioners of the subject.

There has been a reluctance in the conservation profession to employ staff in a purely technical capacity, the feeling being that only a trained conservator can really be entrusted with the treatment of archaeological material. This has, in all probability, weakened the position of the conservator, who has not infrequently had his illusions regarding the more interesting aspects of the work shattered by being tied to the bench treating quantities of material which is mediocre or worse. The whole question of job satisfaction must be carefully considered as training in the subject advances.

Some of the larger museums also employ what are termed conservation scientists, usually with a formal training in the natural sciences, but not necessarily with a training in conservation except, perhaps, in a limited area in which they may have done a higher degree. Such people perform the valuable function of taking an overall view of the scientific needs of their colleagues and may carry out analytical work, the testing of materials and the development of new techniques.

I am personally convinced that there is a need for all three of these categories within the profession: conservation scientists, conservators, and technicians, the number and proportions being determined by the size and scope of the museum or unit concerned. The broadly trained archaeological conservator is, however, the key member of this trio. Many museums, perhaps most, can afford only one member of staff for the care of their collections and it is this category of conservator which will always be in demand.

One of the problems of conservation is that it does not support a sufficiently large economic output to justify heavy investment. The net result of this is that it is difficult to interest commercial organisations to undertake development work, however well justified, because even if successful it would be quite impossible for the returns to cover even the original investment. The truth of this observation is supported by the very small number of patents for conservation processes. This is a great pity, because there are a number of well-understood requirements within conservation, for example the need for adhesives or consoli-

dants with precisely defined properties and a high degree of permanence, which certainly could be produced with little difficulty and which would have appeared long ago had there been an industrial or military requirement. It is true that on occasion large sums have been invested in stone conservation projects, or in the conservation of library materials, but the economic advantages of this are more obvious. Instead, great reliance has had to be placed upon materials and processes developed for other purposes such as the electronics industry, dentistry and packaging, adapted as desired. Nor have many new techniques been developed specifically for conservation, although developments in other fields have been borrowed almost as soon as they have emerged, often with spectacular success. Apart from analytical techniques, X-rays are widely used in conservation for the examination of objects, sometimes obviating the necessity of any other work on an object and enabling the archaeologist to derive all the information he requires from the films. Interest in archaeological applications has also made available on occasion such facilities as the CAT scanner, used for whole-body X-ray tomography. Lasers have been used both for the examination of objects and for cleaning delicate surfaces, gas plasmas have been used for removing deposits from objects which cannot stand chemical or mechanical treatment.

The whole question of research in conservation is a problematic one and I hope that I do not offend anyone if I say that, to my mind at least, there is no such subject as conservation research *per se*. I do not mean by this that no research leading to the better conservation of archaeological artefacts is worthwhile. Far from it. I do believe, however, that with a full understanding of the materials of antiquity and the mechanisms of their deterioration, it should be possible to predict with near certainty how to go about the process of conservation. Understanding of the nature of ancient materials must, of course, be supplemented by an equal familiarity with the behaviour of the natural and synthetic materials used in conservation, in order that the future stability of treated artefacts can be ensured. This is my personal justification for the dual role of the Department of Archaeological Conservation and Materials Science, namely for research into all aspects of ancient materials, and, where appropriate, application of the results to the conservation of artefacts made from them. Almost all the conservation-related research carried out in the Department relates to processes of deterioration, but some work on the evaluation of possible conservation techniques is also in hand.

Training

It was in order to fulfil the rapidly growing demand for conservation staff in museums and on excavations that the Institute developed a formal training in archaeological conservation and, as far as I am aware, it was the first academic institution anywhere to do this. Previously, most museums had employed craftsmen, and occasionally scientists, and they had acquired their skills in part from their colleagues, if any, and in part from published work. This led to museums often having very distinctive 'house styles' in conservation, sometimes still recognisable today – there was much rivalry regarding techniques and standards.

We are most fortunate in having with us today, and indeed chairing the proceedings, the

originator of this training programme, Miss Ione Gedye. Miss Gedye established conservation at the Institute in its earliest days and, during some 40 years under her guidance, the Department's facilities grew from a small outdoor workshop-laboratory at St John's Lodge in Regent's Park to the purpose-built laboratories in Gordon Square which we use today. From a basic technical training the teaching developed through a Diploma course in Conservation, a Certificate Course taught for the Museums Association, shorter courses for the British Council run jointly with the British Museum and ultimately the BSc degree in Archaeological Conservation which, like the earlier Diploma, is recognised worldwide as a professional training in the subject. By the time she retired in 1975, there was hardly a conservation department in the world which had not benefited either directly or indirectly from her teaching, and we believe that students from at least forty countries had passed through the Department. For 11 of those years her colleague in the Department was Henry Hodges, later to teach conservation at Queen's University, Kingston, Ontario, and probably best known to students today for his two books on the history of technology.

Although the BSc degree has become the Department's core course, it has also become necessary to establish two non-degree courses, of a largely theoretical nature, to cater for conservators from overseas who may be of some seniority, but who have no formal qualification in the subject, and also for some conservators from the United Kingdom who are receiving their practical training in their own museums.

It is often remarked that while there are three universities teaching archaeological conservation in the United Kingdom, there are remarkably few elsewhere, notably in North America. One reason for this is the special practical requirements of the subject. Just as anatomy students require cadavers, conservation can only be taught adequately through the provision of a steady flow of excavated and museum objects of all materials covered by the course. In this respect we are extremely fortunate, having access not only to material from Institute excavations and in the museums of University College, but also items from smaller museums in south-east England and sometimes further afield which have no conservation facilities of their own, or who have more material than they can handle themselves. Much material also comes from private sources. In any one year, between 300 and 500 very varied objects, archaeological, historical, and ethnographic, pass through the Department – it is difficult to envisage many other places where this would be possible.

Just as important as in-house practical work is the fieldwork element, which forms an essential and valuable part of the course. All students taking the BSc degree must spend a minimum time working as conservators on excavations or in museums in order to gain experience outside the teaching environment. This fieldwork may be at home or abroad and is often arranged in collaboration with former students who are in a particularly strong position to help in this aspect of the training.

Traditionally, members of the Department have become involved in conservation projects which not only contribute to research and teaching, but which allow students an opportunity to make a personal contribution to the subject. These may be field-based or laboratory projects and have, in recent years, included such problems as the extraction of papyrus from cartonnage; the conservation of the excavated wall-paintings from the Roman

villa at Sette Finestre in Tuscany; the setting up of archaeological conservation and training facilities for a large-scale excavation and conservation project in Sri Lanka, and the conservation of a large group of neolithic lime-plaster figurines from the site of 'Ain Ghazal in Jordan.

The future

Looking to the future, it has become apparent that the potential for future development has become severely restricted by the limitations of space. To the consternation of some of my archaeological colleagues, the Department has already spilled over from its original sixth floor home and established colonies on the second floor and in the basement. The potential for further expansion is nil and desirable developments such as new courses, the provision of a comprehensive conservation library and information service, and increased capacity for contract work, can only go ahead if more space becomes available. As some of you will already know, the Institute decided, as part of its Jubilee celebration, to launch an appeal for a new Conservation Centre. The aim is to provide adequate dedicated space for the Department's needs for the foreseeable future, but it will go further than that. In moving conservation out of the present building, there will be a release of space which will, in turn, allow for the expansion of the other Institute departments and facilities; if it is successful, the Institute as a whole will benefit.

As a final thought, it is perhaps worth mentioning that the study of archaeological conservation does have implications well beyond the bounds of archaeology or museum studies. There are, believe it or not, industrial applications of some significance deriving from conservation, or at least in that aspect of the subject which deals with the processes of deterioration. These lie in the prediction of the behaviour of structural materials over extended periods of time. Techniques such as accelerated ageing are no substitute for the study of deterioration under normal conditions; information derived from archaeological artefacts, both terrestrial and marine, is of value for predicting the performance of pipelines, oil rigs, and nuclear waste containers under different environmental conditions. I forget what percentage of the gross national product is lost through corrosion alone, but it is an almost astronomical sum in cash terms. It is in areas such as this that the kind of conservation which I have been talking about this evening meets that of environmental and resource conservation.

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Biophilosophical Aspects of Archaeology

by DON BROTHWELL

Introduction

In view of the fact that both the Institute and I were born about 50 years ago, give or take a very small carbon-14 error, both events would seem an appropriate excuse for me to 'wax philosophical' about the nature of human life in relation to archaeology. Actually, my links with archaeology go back about 40 years when, as a young boy, I visited Trent gravel works near Nottingham, where I fished for sticklebacks and initiated my comparative bone collection from the finds being dredged up. Links with the Institute might be said to have started, somewhat tenuously I admit, when about 15. To my youthful indignation, I found that the local Iron Age fortification at Breedon-on-the-Hill had only briefly been excavated prior to increased quarrying, and I set about further rescue work on the Saxon cemetery above the quarry face. Later, I was to find that the lady who all too briefly excavated Breedon (Kenyon, 1950), was far more concerned with Jericho, but while attending her classes, I never mentioned the Breedon incident. In fact my amateur diggings there produced the skeleton of a child showing evidence of Down's syndrome (Brothwell, 1960), the first clear archaeological evidence that an earlier community could be compassionate and protective of their mental defectives.

While assisting in my teens at another site, I was introduced to a gangling blue-nosed man, who was later to teach me British and European prehistory. His *Dawn* (Childe, 1950) was the worst textbook I consulted as a student, but his kindly eccentricity showed me the better side of academics, and his later suicide was impressive in demonstrating how fragile human judgement is. My arrival in London to study a very mixed bag of archaeology, anthropology, vertebrate zoology and geology, followed a formative, Quaker-influenced period in Lincoln Prison. Sixteen centuries earlier Maximilianus, son of a veteran Fabius Victor, was one of a number of young Romans who declined to take part in Roman militarism (Toynbee, 1957), but, like some of my predecessors in the First World War, execution was to resolve his problem of conscience. The dehumanising effects of urban cultures indeed have a long history. To me, war and human aggression are still major issues, and they clearly have a prehistory, but it appeared to be a topic of no great relevance in my archaeology courses. I still remember a stimulating talk by Phyllis Kaberry, a social anthropologist at UCL, on the ritualised and relatively violence-free warfare of the New Guinea Abelam. I was impressed that a tribal society, all too often viewed as 'savages', had evolved an advanced adaptive strategy to minimise actual physical damage between groups. How different was the tone in

Man Makes Himself (Childe, 1948), written by that kindly communist with the right-wing style of a Winston Churchill. 'Militarism', he wrote of the Ancient East, 'was necessary to protect the achievements of civilisation against the envious attacks of slothful barbarians.' He does confess, however, that 'As a civilising force, militarism's record is . . . disappointing'.

While I was struggling with Iron Age pottery, fossil hominid skulls and the genetics of mammals, I should mention another event which impressed me, but which remained a non-curricular matter. A fellow-student displayed irrefutable evidence of telepathy and ever since I have remained interested in the controversial field of parapsychology. While scientific caution is necessary, closed minds are not, and I would still maintain that if telepathy and certain other paranormal phenomena receive further substantiation, then archaeology may be unable to leave them out of any future consideration of the transmission of ideas, even cross-culturally.

That, perhaps suitably, brings me round to the nature of this Jubilee Lecture. While spending much of my teaching and specialised research time in a restricted part of environmental archaeology, I want here to extend the term to include human social environments and raise some problems concerned with this much broader canvas of environment, especially in relation to aspects of the biology of the hominids. This has not been an easy domain to move into, even though some of the problems have long interested me. If colleagues with deep involvement in cultural archaeology find some of what I have to say trite or absurd, it will at least have demonstrated the need for closer integration of bioarchaeology with archaeology as a whole.

Let me begin with the brain.

The problem of the brain

In *The Descent of Man* Darwin writes, 'As the various mental faculties gradually developed themselves the brain would almost certainly become larger. No one, I presume, doubts that the large proportion which the size of man's brain bears to his body, compared to the same proportion in the gorilla or orang, is closely connected with his higher mental powers' (1890 edition, p. 54). In fact biologists have been impressed with our brain size and apparent intellectual abilities for a long time, and a century before Darwin's comments Linnaeus had bestowed on us the species name *sapiens* as something of a biological accolade. This arrogance of giving such a name to our own species was at least scaled down by the fact that great apes enjoyed the same generic name of *Homo* (*H. sylvestris* and *troglodytes*). Regrettably, the sapient name was not changed when these two pongid species were demoted and the name *Homo* removed.

It should be noted that various mammal groups display a trend in increasing brain size during the Tertiary period (Jerison, 1973; Eisenberg, 1981). The reasons may vary in different groups and, while in the carnivores and bats, it appears to be related to feeding strategy (Gittleman, 1986; Eisenberg and Wilson, 1978), in chipmunks it is linked to contrasting habitat complexity and thus different behavioural demands (Budeau and Verts, 1986). Distinctive among mammals are the primates who particularly show the evolutionary develop-

ment of a brain which is large in proportion to the total body weight, and in the hominids displays an extensive and well-convoluted cerebral cortex (Le Gros Clark, 1959). The apparent adaptive strategy of increasing relative brain size is thus a mammalian, and especially a primate characteristic, not simply a hominid one, and there is certainly no cause to believe that the large, convoluted hominid brain is anything other than the ultimate extension of the primate trend. There is no doubt, however, that compared to any other primate genus (as far as our knowledge yet goes on fossil forms) advanced hominids have particularly large brains.

Thus, while Darwin is likely to be right in equating human mental abilities with a particularly well-developed brain, it seems debatable whether one can simply presume that eventual behavioural attainments dictated brain size increase. At least it seems to me that further debate is needed on the nature of human behaviour and its evolution in the hominids in relation to brain size. Perhaps it is indeed a safe assumption to link behavioural needs and changes with brain increase, in a simple cause and effect relationship, or is it? In modern human brains, one is dealing with a highly complex structure of some twelve thousand million cells with an elaborate pattern of interconnections (Rensch, 1972). It is in the organisation, and probably not in numbers or density of cells, that the hominids show their distinctiveness. Knowledge of this vast system, of its genetics and micro-functioning, is still relatively elementary. If we take a commonly occurring mental problem, schizophrenia, there is clearly unreliable functioning, perhaps with a metabolic basis, but still nothing certain is known of this distressing condition.

Within our species at present, brain size varies by as much as 1,000 cc's, and there is no clear relationship between intelligence and size. Elliot Smith considered that the brain had to be about 950 grammes (*c.* 1,000 cc) to be normal (Keith, 1929) but there are reports of intelligent members of European society who have made do with less.

Because the majority of a population develop a brain size which is perhaps up to a third more than is needed for the normal survival of the population, it does not necessarily mean that this brain size was not at times a critical factor in coping with periods of stress. Could it be that in a species such as our own, where culture is now important to our survival, the maintenance of an excessive brain size in the majority of a population may ensure that at least a few will produce the right degree of creative thinking at a time of crisis? 'Over-investment' of this kind could surely only be a good strategy. This hypothesis would make sense in our own technological society, but is it a reasonable explanation of brain increase in the early Middle Pleistocene? Were selection pressures on behaviour in the Early Pleistocene so strong that extra brain size was critical? If one could demonstrate material culture contrasts of the order we see in Bushmen and our own 'advanced' micro-chip societies, there would perhaps be little reason to doubt. But prior to the more advanced technologies of the Late Palaeolithic, what is there to show in social or cultural terms for the brain size changes? It is easy enough to invoke a variety of behavioural changes which sooner or later took place. Changing resource exploitation, including perhaps developing skills in hunting, is one possibility, although Binford (1981) doubts this as an Early Pleistocene change. Increased sociability in other respects seems likely, but with the perspective of a range of social behaviour in other mammals, it is difficult to see how modest social changes would be critical determinants of significantly larger brains over a

million years ago. Language has been discussed by many as a critical factor. Medawar (1976) points out that the hominid characteristic of 'cultural heredity' is dependent on language to a significant degree. There are, however, considerable differences of opinion as to the antiquity of language in the primates, with Rensch (1972) suggesting that the proliferation of language would only have been useful at the phylogenetic level of *Homo erectus*, and that languages that had developed words for causal and logical relations should perhaps be attributed to the Neanderthalers or even later *H. sapiens*.

In considering this hominid category of 'cultural heredity' one presumes that the complex symbolisms of religion and art would have had no adaptive relevance at the early *Homo* level. In the present state of our knowledge about human aggression and the development of warfare (Brothwell, 1985) it seems unlikely that strife of this kind would have acted as a significant differential in favour of brain increase, but this is a controversial issue resulting in a considerable literature. Ferrill (1985) suggests that it was only in the Late Palaeolithic to Neolithic phase that very significant changes took place in 'weapons technology' which could have influenced the development of warfare. Certainly, there is no suggestion in previous tool technology of special weapons for aggression, although wooden hunting spears clearly have a prehistory back into Middle Pleistocene times. Some behavioural scientists (Eibl-Eibesfeldt, 1979) believe that 'aggressivity' is a constitutional characteristic of man and believe that there is limited evidence for human violence in the Palaeolithic, but no one has suggested that this would provide a significant differential in favour of brain increase in the earlier Pleistocene. In terms of brain increase as environmental adaptation, Konrad Fialkowski has provided perhaps the most unusual explanation. He points out that studies on thermohomeostasis on birds and mammals seem to indicate that 'the main adaptive significance of this temperature regulatory mechanism is the maintenance of a constant temperature of the brain', temperature influencing enzyme efficiency. The argument then runs that hunters in the tropics are especially susceptible to heat stress and this might have resulted in brain cell damage. There was thus a positive advantage in having more than enough brain cells. The problem is that we do not know the extent to which Early Pleistocene hominids were hunters or heat stressed and, in view of the spread of hominids into many parts of the Old World by Middle Pleistocene times, significant brain increase could well have occurred in cooler climates.

Most recently Nicholas Humphrey, a psychologist, has become interested in the contrasts between the apparent existential ignorance of most primates and the self-reflective intelligence of our species. He argues that even in higher primates, while they seem mainly to eat, sleep and play, in reality their lives are different, and we under-estimate – he thinks – their social problems (Humphrey, 1986). He suggests that there are pressures to maintain stable social groups, which have survival value in providing 'both a protective mafia and a kind of polytechnic school' all in one. His argument then is simply that humans have taken this 'social intelligence' a stage further, perfecting what he calls an 'inner eye', in order to contemplate as well as possible people and the world 'from the inside' of the mind.

Pondering this hypothesis, and remembering that there are other social mammals and that some of these groups show tendencies to brain increase in certain circumstances, it still seems to me that this 'inner eye' is the product and not the trend-setter which resulted in such

large brain increase. As we are relatively unique in terms of our brain size in mammals, could the critical initiating factor also be unusual? Have we been sitting on the answer all along? You will recall that early fossil hominid evidence shows fairly clearly that an adaptation to an upright, bipedal posture came well before significant brain increase (McHenry, 1975). The evolution of larger brains by the *Homo erectus* level was thus later; clearly one needs to question whether this was another entirely separate event, related to different selection pressures, or whether it might have been secondary to other aspects of hominid growth? In the past, I have viewed it as separate, but now I have my doubts. In the evolution of bipedalism, the main remodelling was to the pelvic area and the thighs. There were also other related changes, for instance, in the foot and in the curvature of the lower spine. Could there have been further spin-off from such a unique primate event? We know from the badly formed pelvic basins of women with rickets, how critical it is to have a well-formed birth canal for the passage of babies. When pelvic changes were part of a progressive adaptation, clearly there would have been survival advantage in remodelling which allowed for ease of birth, especially of the slightly above-average head. Very small advantages of this kind, through time, could have set a trend to larger brains, especially if, as now seems likely, there have been various changes in growth rates – including of the foetal brain – during hominid evolution.

I have dwelt somewhat on these problems of the brain because it seems to me that attitudes to the importance of the brain in evolution colour our interpretations of hominid cultures. I suggest here that, contrary to current opinion, the brain in the earlier Pleistocene may have had very modest importance and that only later, by Middle Pleistocene times, was there significant reciprocal feedback from cultural developments acting as selective pressures which would have further enhanced brain increase. This more limited time-span available for behavioural adaptations, may better explain our social limitations and perhaps our unique development of a range of social pathology. If our present brain size represents strong behavioural selection right through the Pleistocene, then why are our societies still so appallingly defective or maladapted? Where is the wisdom of our 'inner eye' in a world evolved to absurd politics, nuclear weapon madness and religious conflict and with an archaeological record which suggests that social abnormality has been a recurring theme for millennia?

Archaeology, ethology and sociobiology

In our consideration of the brain, we inevitably move on to questions of behaviour and its evolutionary significance. At the time when the Institute of Archaeology was being created, developments were taking place in biology which were to have significant repercussions in the human sciences, including archaeology. At that time a number of zoologists, of whom the most well-known was Konrad Lorenz, were defining the field of ethology from a growing range of studies on animal behaviour. In fact 1937 saw the first volume of the German journal of animal behaviour (*Zeitschrift für Tierpsychologie*), and other similar journals followed. In 1975 E. O. Wilson, a zoologist at Harvard, proposed a change of emphasis in behavioural

studies, thus creating 'sociobiology', which can be defined as 'the systematic study of the biological basis of all social behaviour'. In effect, this 'Darwinises' ethology, and states that all significant behaviour is largely under the control of genetic pre-programming.

Resistance to this all-out evolutionary takeover of behaviour has, of course, been considerable, and it has taken 10 years for the human sciences to be stormed by this new concept, with archaeology still not realising the significance of these events. So does all the great variety one sees in past cultures essentially boil down to our genes promoting any behaviour which has survival value? Well, within the decade, sociobiology has matured considerably and is still adapting. It is now accepted that human behaviour results from both genetic and environmental factors (Chagnon and Irons, 1979), but gone are the days when archaeologists or sociobiologists could view humans as culturally 'blank slates' at birth and that our behaviour is pure cultural imprinting. Clearly, the situation is very complex and I believe that archaeology has a real contribution to make to understanding the mosaic of interactions between biology and culture. Of course we can go on strictly separating these two aspects, and consider cultural evolution as 'a very rapid Lamarckian process' (Wilson, 1979) but is it any longer sensible to exclude significant biological links if they are being established?

Critics of the importance of sociobiological studies have pointed out that there are plenty of examples of cultural variation, practices, beliefs, mythology, where it is difficult to see any survival value in the characteristic. Nevertheless, surely it is pertinent to ask how cultural complexity fits in with the broader adaptive needs of hominids to store information of all kinds in transmissible form? Lorenz (1977) rightly points out that human knowledge can only be passed down cumulatively over substantial periods of time if it is in a suitable 'inheritable' form, that is, has been incorporated into firm structures within cultures. Because culture parallels biological evolution, in that traits may be affected by what can be similarly called inheritance, mutation, selection, drift and isolation, it is no wonder that some characteristics appear and eventually disappear without obvious survival value. If we look at the literature on the use of thallium therapy in medicine, we see that it was an innovation in about 1914, but its impact was very modest until 1925 when, for uncertain cultural reasons, it became a medical craze, fading out as sensible therapy by about 1940 (Penrose, 1952). As far as I know, no genes were lost here, and what we see is some innovative thinking cashing in on a part of society and being carried along without survival value until replaced by other ideas and vogues. Could one, in the same way, view the cultural career of European beakers, or the varieties of 19th-century marbled pop bottles, as cultural mutants with no long-term survival value? Archaeology, under the stimulus of sociobiology, should certainly be looking more closely at the enormous cultural variety it is privileged to handle and ask what the potential survival merits might be of more lasting or recurring characteristics. On the evidence of other species it is clear that the transmission of at least some behavioural patterns in hominids is reinforced by deep-seated neurophysiological impulses, and it has been shown that when thus established, deviance from habits in various species results in anxiety reactions. Anyone doubting the importance of these subconscious controls need only recollect that anxiety neuroses are some of the common major health problems of advanced societies and are no doubt linked to our social disintegration. This whole question of conformity is clearly deserv-

ing of more attention, not only in modern societies, but as a major theme of relevance in explaining past trends and traditions.

In pondering all the problems from complex brain cell activity encoded in the gene complex through to the less-known ramifications of culture trait adaptation and transmission, Lorenz (1977) has been impressed by the probable importance of what he calls the 'creative flash' in our evolution. Could it be, in fact, that what encouraged the emergence of our own large brain in late Middle Pleistocene times was increased challenges, sparking off more creative effort? For instance, once language was on the move was it stimulated by enhanced logical and abstract thought, which then acted as a drive for a more elaborate language and thus further exploration of a world more open to group debate under the ever-increasing 'control' of language? One might recall the blind and deaf mute, Helen Keller, whose inborn capacity to learn, combined with the skilled prompting of her teacher, resulted in rapid expansion of a vocabulary, once she had realised the significance of the language symbols. Beyond the creative flashes and cultural exploitation, adaptive pressures on some part of this range of behavioural activity would have consolidated it at a phylogenetic level.

There is, of course, a very sobering side to all this. Evolution is unfeeling and has no purpose and, although there may have been significant figures in history and prehistory, in the long-term 'culture is unresponsive to the wishes of individuals' (Alexander, 1979). Had the Nazi Germany which sacked Frederick Zeuner and rejected his wife been larger and economically strong enough, it might still be in control of a vast oppressive empire in Europe today. The reproductive success of these fascist groups would have been ensured, and the cultural monster could have had a biology as impeccable as a *Tyrannosaurus*. So, if laws of chance and necessity control all aspects of our evolution, biological and cultural, then is life really rather a swindle? Is the past only a record of the long, meaningless, meandering evolution of a peculiar culture-ridden species? If so, perhaps the works of Shakespeare might just as well have been written by the proverbial monkeys at the proverbial typewriters! Are even the ethical codes which have evolved in relation to human societies nothing more than roundabout ways of protecting the omniscience of the selfish gene? Oh, God, the Father and the Gene! But no God is needed in this argument.

In his recent book in defence of evolutionary theory the Oxford zoologist Richard Dawkins likened the variety of the life forms produced in the unfeeling process of evolution to the creative work of a blind watchmaker. While this 'biopoetic' analogy seems basically appropriate, there is one factor in our own evolution which is both remarkable and puzzling – our special state of self-consciousness. It would appear that we alone have this highly developed capacity to reflect on ourselves, our separateness and the universe about us. Incredibly, this particular timepiece created is looking knowingly at the blind watchmaker! What is this phenomenon of self- and universe-consciousness? While of the brain, it extends uniquely beyond the brain, and is responsible for a long history of human compassion and true disinterested altruism which extends well beyond the interests of the replicating gene. This surely is the major enigma of biology and history. One might hope that archaeology, while sensibly embracing sociobiological theory as it must, will nevertheless ponder with its long temporal canvas of facts this unique phenomenon of our species.

Civilisation

It is nearly 100 years since the socialist reformer Edward Carpenter (1889) published his book *Civilisation Its Cause and Cure*. 'We find ourselves today', he writes, 'in the midst of a somewhat peculiar state of society, which we call Civilisation, but which even to the most optimistic among us does not seem altogether desirable. Some of us, indeed, are inclined to think that it is a kind of disease which various races of man have to pass through ... but ... while history tells us of many nations that have been attacked by it, ... we know of no single case in which a nation has fairly recovered from, and passed through it, to a more normal and healthy condition.' In view of all the developments since then in archaeology, not to mention psychiatry and sociology, one might have expected that the Wolfson College lectures of 1978, on the 'Origins of Civilisation' (Moorey, 1979) would have considered current hypotheses on the causes and nature of early civilisations. Were overall cultural changes related to environmental demands, changing levels of technology, territory and population pressures, political and economic factors, or what? And what is 'Civilisation'? The specialist papers in this report were a complete cop-out and we were left to make up our own minds about what we thought this concept 'civilisation' meant.

I presume most archaeologists use the term to mean a level of material, cultural and social complexity which contrasts with the kind of tribal society likely in the Palaeolithic and Mesolithic. Moreover, as Toynbee (1946) points out, '... civilisations are not static conditions of society but dynamic movements of an evolutionary kind' (Vol. 1, p. 176). However, as a result of Karl Popper's (1957) attack on 'historicism', it is of course now sensible to be cautious. 'It is a mistake', he writes, 'to believe that there can be a history in the holistic sense, a history of "States of Society" ... this idea derives from an intuitive view of a *history of mankind* as a vast and comprehensive stream of development. But such a history cannot be written' (p. 81). The answer to the problem, Popper suggests, is simply to 'introduce a preconceived selective point of view into one's history' (p. 150). While this advice is no doubt a great relief to many archaeologists, who simply want to remain site- or period- or material-orientated, to me it seems to amount to yet another sort of cop-out. Surely there is a need to give preference and priority at this stage in the development of archaeology to problems related to recurring themes in the subject, environmental, technological and especially behavioural. In particular, it is time to ponder the very nature of our species.

It has always struck me that 'civilisation' is a somewhat elitist term, which is potentially derogatory to those groups not included in its definition; one of the multifarious ways of socially or linguistically 'failing' people. Yet in terms of the mental quality of life for the majority, what real improvements can we define in early, or for that matter more recent, civilisations? While complex language was established well back in the Palaeolithic, writing is certainly linked with urban development and the increased interest in perpetuating information, but relatively few have been literate in the past. Urbanism may simply reflect demographic pressures, not only in terms of population in relation to resources, but increasing territory restrictions (Binford, 1983). The adaptive strategies called agriculture could have relied on very ancient wisdom of the living world, and thus be no more than clever technological innovation from long established knowledge. Urban density may be a reflection of

growing fear of violence and thus strategies of group defence; as much as religious or political developments. If we interpret the reasons for Neanderthal burial correctly, 'religious' attitudes were evolving long before the end of the Palaeolithic, reinforced no doubt by skilled visual art, music and dance. In other words, although there is clearly a change of gear in terms of *technical accomplishment* in the Holocene – possibly all linked to demographic and environmental pressures – can we really say that there was any significant difference in advanced *Homo sapiens* through time? I doubt it, and except in one or two serious respects, human experience may well have been fundamentally the same back into the Upper Pleistocene. Any really meaningful differences are concerned with the possibility that cultural development, especially as regards social interaction in urban situations, has progressed too fast for human nature, our inherited behavioural potential. Can we strip away the material superficialities of past urban cultures and begin to speculate on what may have been happening on the level of human feelings? A little more effort in this respect seems to be long overdue. Urban societies, by their very hierarchical nature, impose constraints on substantial parts of the community, and thus one can reasonably question whether frustrations and anxiety states may have become commonly occurring aggravations. Neither rats nor people tend to be on their nicest behaviour when crowded together! In marked contrast to tribal society, it seems to me that at most stages of urbanism, individual freedom would have been commonly restricted and demands on labour extended to the extent that the quality of life was all too often barely tolerable. Recurring states of mental distress at an individual level would have helped to initiate various forms of social pathology, violence and social disintegration. The fully integrated individual of the smaller tribal society was more and more replaced by the increased alienation of each person in the specialised and regulated urban society. Elites, with their concern for material wealth and biased distribution of resources, consolidated their position by social techniques and deceptions. In such a climate of exploitation and frustrated expectations on the part of most ordinary individuals throughout the last few thousand years, to what extent should we be reconsidering the development of religious and other social phenomena in terms of adaptive strategies to encourage internal social cohesion. Similarly, does it make any sense to postulate the collapse of 'high cultures' in terms, not of an adrenalin 'burn-out' on the part of the community, or even catastrophe theory, but rather because the people were pushed beyond a critical threshold of tolerance, resulting in a generally depressed morale, reduced physical support for the ruling groups, and perhaps even violence or degrees of non-violent disobedience. The history of urbanism is, if nothing else, a lesson in the power of domination of the majority and their indoctrination into the acceptance of a wealth-supported elitism. What began as the development of resource-exploitation strategies for community survival was soured by the opportunistic behaviour of more extrovert and dominant members of these communities. Their genes and families survived well, but the systems which developed were malignant, and still are.

Of war and peace

There is little doubt that one of the main forces in the history of so-called civilisations has been the emergence of ever more elaborate techniques of warfare, with greater and greater calls on

creativity and resources, and greater degrees of destruction. Toynbee (1946) is in no doubt that militarism 'has been by far the commonest cause of the breakdowns of civilisations during the last four or five millennia'. It is in warfare also that we surely see the tragic limitations of most people, from prehistory to the present, to perceive the absurd reality of socially dictated mass killing. We are viewing an inability of culturally imprinted people to react positively against social pressures to take part in what is social pathology. Deep-seated reactions, called 'conscience', are very uncommonly a primary response, but this depends on culture and attitudes to violence. There is an account in Albert Schweitzer's writings of how the local tribal community living near his African hospital reacted to news about the First World War. He writes (Schweitzer, 1955) that of the Europeans who returned home to military duties, news was received that ten had already been killed. The tribal Africans were impressed, and one old man remarked 'Ten men killed already in this war! ... Why then don't the tribes meet for a palaver? How can they pay for all these dead men?' Little were they to know that it was to cost Europe about ten million deaths and twice as many wounded, and for nothing.

While each war is different, the basic controlling formulae are likely to have been the same back into prehistory. Archaeology and history provide an important temporal extension to the 'statistics of Armageddon', and enable questions to be asked about the possible long-term inherent aggressiveness of human communities, and whether some situations promote war, and others peacefulness.

Although we use the terms war and aggression commonly, there is in fact considerable confusion in the use of such terms. What we refer to as aggression in primates can vary from facial posturing, implying 'keep your distance', to physical violence – especially in situations linked to the establishment of male dominance. It is also known from experimental work on Rhesus monkeys, that offspring deprived of attentive mothers show behavioural defects, and their offspring may display abnormal violence even during infancy (Sackett and Ruppenthal, 1973). We may well ponder whether variation in mothering attentiveness in our own species has similarly resulted in a later harvest of delinquency. But, clearly, aggression in hominids is a far more complex phenomenon than in other primate groups. If we continue to use the term in its present general form, then it clearly must embrace everything from day-to-day externalised frustration, confrontation and anger within families or groups, to the mentally confused violence of psychopaths. Set within social environments which include such behaviour, most of us could be counter-aggressive, but I think it incorrect to thus conclude that there is a well-defined inherited basis for violent aggression in us all.

The second category of aggression is the between-group animosities linked to overlapping resource or territorial interests, political or religious differences, or perhaps other claimed social outrages of one group against another. These sparking-off factors must have changed considerably in importance through time, and there is surely little doubt that the emotional content of such conflict has declined through time the more organised it became. In fact, it may be sensible to reserve the term warfare for the type of conflict we see evidence for in urban societies developing over the past seven or eight millennia. The communities were evolving social mechanisms with the power to gather large numbers of individuals together, with increasingly specialised equipment, for the purpose of suppressing and often killing large

numbers from other communities. While fear must have been well present in such conflicts, they must have become increasingly dispassionate. The use of language combined with social distance between communities has ensured, through much of the history of warfare, that the 'enemy' were evil, if human at all. There is a coldness in some early accounts which shows the extent of the social pathology. The Assyrian King Assurnasirpal wrote: '600 of their warriors I put to the sword; 3,000 captives I burned with fire; I did not leave a single one of them alive to serve as a hostage' (Butterfield, 1981). The fact that captives were impaled on spikes or their heads hung on trees, suggests that visual terrorising was already being developed as an intimidation technique even in such early societies.

While warfare in these early communities may have conferred temporary reproductive advantages on one side, and stimulated cultural innovation, as an evolving behavioural phenomenon it represents the most serious kind of maladaption with which we are confronted in archaeology. I should say that this is not the interpretation the behaviourist Irenaus Eibl-Eibesfeldt (1979) would favour, and he is reluctant to view war as a hominid abnormality. To him the problem is that later hominids have superimposed what he calls a 'cultural norm filter', which encourages killing for gain, upon a 'biological norm filter', which is for the conservation of life. Moreover, he says, 'the root of the universal desire for peace lies in this conflict between cultural and biological norms', which makes individuals want to resolve these conflicting biological and cultural pressures. This avoidance of the term 'social pathology' seems to me another cop-out. If the killing and wounding of perhaps 70 million young men, just in this century, does not provide clear scientific evidence of widespread culture-induced psychopathology, then it is difficult to know how the academic community in the human sciences can ever hope to be taken seriously.

It is interesting to recollect that at a time when warfare was becoming an all-too-regular phenomenon in early human societies, there was also the development of philosophies of peace and compassion. Both Jainism and Buddhism of the fifth century BC independently evolved the ethics of *ahimsa*, compassionate non-violent action with the avoidance of killing, even of other animal life. To the east of these religious movements, mystical Taoism was to emphasise the wisdom of non-violence, and warn of the heady, toxic effects of power on people. Again, by the fifth century BC, the *Chuang-Tzu* records that pacifism was becoming dynamic in China, wishing to unite people in a universal brotherhood (Watson, 1968; Ferguson, 1977). Contrary to the usual interpretations, I would not view these movements as indicative of a new social enlightenment which also appeared in some subsequent religious philosophies. For it seems to me as logical to view these philosophies as a recapitulation of much earlier attitudes, which indeed could have occurred even in Palaeolithic societies, with their probable homogeneity in resource availability, and lack of established aggressive hierarchies, and their lack of interest in wealth for wealth's sake. Could the relatively low variation and slow change in Pleistocene tool technologies be considered in fact as evidence of long-term adaptive stability with few conflict challenges?

The reason why all these peace endeavours over the past few millennia have failed is not that they are set against the inherited aggressiveness of hominids, but because they have not yet found a solution to the problem of competing against the power and domination of self-

interested hierarchies, the perennial temptations in urban societies of the material 'good life' or, if nothing else, by the apparent quicker short-term solutions of organised violence. One could say that the cultural programming of violence is not too well superimposed over basic phylogenetic peacefulness, and in a world of nuclear weapons, time is running out.

Conclusions

Let me conclude this philosophical meandering around archaeology. My concern here has been to discuss a few of the interesting issues which seem to me to deserve further attention. Beyond the basic day to day business of archaeology, it seems to me that the subject wrestles with some problems of immense interest to people in general. Moreover, there is even a chance that a better viewing and understanding of our past may assist in the reorientation of our present alienated society.

It would have been good to consider various other issues, and perhaps I might finally leave you with two or three of these to ponder. I have been aware that in talking of the human brain and behaviour, I have made the assumption that hominids evolved and maintained the same standards of rationality over a long period of time. Similarly, it is assumed that we all have the same sort of experience of reality, but it could be that both assumptions should be challenged. There are a number of associated problems here. First of all, if *clinically recognisable* states of mental disorder can be as high as 20%, even in less stressed rural groups, then what is the nature of reality to many people in such groups, let alone in widely contrasting cultures through time? In fact psychologists, psychiatrists and philosophers have all debated the extent to which humans submerge part of their reality information, possibly to reduce the pain which has to be tolerated by the conscious levels of our minds. And it appears that, for the same reasons, people can distort their perception of events to fit group needs, what Giora Shoham (1974) calls 'Collusion in illusion'. However, there is no doubt that we have got to start to recognise unreality in the make-up of societies (Fromm, 1980), not only today, but in the past.

To wish to expose reality is to wish to face the truth about ourselves, and to see the world as it really is, selfish, anxiety-ridden or whatever. It is surely also to search for the ultimately good things in the nature of us all, which surely is a move towards improving ourselves? It is realising that freedom is a common need, that true humility is a rare but great virtue, that no person is more important than another, that mystical states of 'being' are infinitely preferable to the object-orientated states of 'having' (Fromm, 1979). These are problems for us all to think about personally, but I would argue that they are also extremely relevant to viewing the past with the right sort of perspective.

Ultimately, when all is said and done, it is perhaps in fleeting moments of joy, in visions of beauty, in hardly definable transient mystical states, that we know intuitively what the human struggle through time is all about, and that life is indeed worth living. One is reminded of an old Bushman story (Turnbull, 1984), and perhaps I could relate it to you, as a suitable ending to this discourse.

'A Bushman child, drinking water from a clear waterhole, saw in the shimmering

surface the reflection of a beautiful bird. It was the most beautiful thing he had ever seen, but, looking upward, he knew that the bird had already gone. The boy decided that he had to follow and find it, so off he set. He sought it throughout his adolescence and throughout his youth . . . far from being criticised for abandoning his adult role as a hunter, it was recognised by his fellows that the young man was contributing effectively to society by pushing both his faith and his belief, as he saw them, to their maximum . . . the quest continued throughout adulthood and the hunter, who has become a hunter after truth, is always one step behind his quarry. Village after village tells him it has just left, heading northward. In his old age, the hunter reaches the lower slopes of Mount Kilimanjaro and is told that the bird has been seen high up on the snowy summit. With the last of his strength, the old man . . . climbs laboriously up the mountain side. Nowhere does he see any trace of the great bird he has devoted his life to finding.

'Finally, he reaches the top and knows that his quest is over. He lies down to wait for the end, recalling the vision of his childhood, content with a life well spent, for he had been lucky enough to find beauty once, and in his heart he had never lost it. As he closed his eyes for the last time on an empty sky . . . and stretched out his arms in a final gesture, his opened hands upturned, down from the sky came a solitary feather and settled in one hand. The hand closed slowly, then held it as tightly in death as the vision of beauty had been held during life.'

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Thinking about Archaeology

by RICHARD REECE

Archaeology, according to some people, thrived as an art and is being strangled by science and the scientific approach. According to others it is the injection of logical thought and scientific method which has brought the subject to form a respectable part of modern thought. Yet others feel that it is above all a free mode of enquiry which ought not to be too rigidly defined and whose ways of working cannot be codified. This gives an element of confusion to any discussion of methodology and causes chaos in any attempt to set standards of archaeological excellence. If anyone were to set out to explore this mess it would have to be done on a purely personal level with no illusions about setting out a final analysis of the subject to date, or delusions about the possibility of establishing once for all a sound methodological basis. Whatever any one individual says, or any conference or committee decides, the practitioners of archaeology will continue to amble on in the way that pleases each one most and the subject will therefore continue on its cyclical course as fashion succeeds fashion. This sort of statement is variously taken as cynical, defeatist, disillusioned or unhelpful; I disagree, for I see it as a fairly simple and easily verifiable statement of the obvious. Unpalatable as a statement of the obvious may be, I find it a good starting point for a series of personal reflections on archaeology and the study of the past.

Most archaeologists would be upset to be told that their work was not logical and many would be annoyed to be branded as unscientific. These are perhaps good subjects with which to begin. They cannot, presumably, be using the word logic in its strict and formal meaning of formal or symbolic logic. If they are hoping for this sense of the word and want their work to have the watertight progression from premise to unshakeable conclusion such that not to accept the conclusion, given the premises, would be contradictory, their hope is almost certainly in vain. Many philosophers would wish to restrict this form of argument to formal logic and mathematics. Unless they can reduce all that they wish to say to numbers and then follow only the formal rules of mathematics their quest for formal logic is doomed to failure. It might be possible for a study devoted solely to material to be couched in numbers. It may well be possible for those numbers to be manipulated in a way which is satisfying to the operator, but the onlooker who wants a human result to grow out of the process is likely to regard it as nothing more than a mathematical variation on glaringly obvious observations.

If work is not conducted according to formal logic can it still be regarded as scientific? Or, is work branded as unscientific if it is merely reasonable and not formally logical? Again, there seems to be something approaching unanimity in the philosophical view of science as a subject concerned with reasonable certainty through avoidance of contradiction rather than

formal demonstration or proof of its theories. So the fact that archaeology can probably never be practised according to strict logic does allow it still to be scientific. What would a scientist expect to see an archaeologist doing if his work was to be called scientific? There should be a familiarity with the observations made to date, or at least with their categories and areas of interest. From the body of data people should select areas for special attention and from this attention there should grow up ideas in the mind of the archaeologist which would put the observations into relationship with one another so that they become articulated parts of a mental whole which can react on one another without causing contradiction. When the idea has been roughed out, and its obvious inconsistencies with what is known have been corrected, then it is time to search for new information in the same line of study against which the idea can be tested and, if the new information gained does not upset the theory or idea, then, for the moment it may be taken as useful and, in a certain limited sense, valid. The scientist does not select facts already to hand from previous work which themselves will produce a theory for him, he selects facts which help his own mind to think up the theory. The theory does not therefore grow logically (by formal logic), necessarily (in the sense that the theory is demanded by the facts) or uniquely (the only possible theory) from the facts; nor is there a formal demonstration of the theory from the facts, nor is theory proved by the first facts, or the facts later amassed. The theory remains the best possible explanation provided reasonable trouble has been taken to test it; it remains the best possible explanation until either facts which arise or are searched out conflict with it, or another theory or idea is constructed which seems equally consistent with the known evidence. Ideally this is a messy state of affairs because further progress on this area of study is hampered until one of these theories has been eliminated or, at best, made to seem less likely than the other on the basis of hard material observation. I find opinion of theories pointless, unless it is restricted to admitted subjective response – that theory gives me a warm feeling – the only possible criteria by which to judge theories are verifiable observations. I suspect that theories cannot be judged by other theories. It may be possible to reach a point at which it is obvious that two theories cannot be held simultaneously, but it seems quite unsatisfactory to hold to a preferred theory and dismiss an unpopular one; the cause of the incompatibility must be found by referring to the observations on which the two theories are said to be based. This may or may not be a good description of the scientific method in its philosophical arguments, but it is, I submit, a reasonable description of the process, and it is worth comparing the practice of archaeology with this picture of 'being scientific'.

The comparison between archaeological practice and the picture gives very little hope to those who would want to describe their subject as 'scientific'. There are pieces of work which follow these lines but they are rare, are not held in particularly high esteem, in fact are often dismissed as 'pure research' – with the implication 'not of much practical application' and they usually occur as isolated forays into research rather than being part of a unified whole. Most of the archaeology that is done happens because there is some outside pressing need, such as the destruction of a site or the need for a catalogue in a museum; very often the only excuse for the study is 'because the material is there'. This suggests that archaeology is to a science as butterfly hunting is to entomology. A large question with this sort of analogy concerns the

nature of the science of which archaeology is the bug-hunting fore-runner. If archaeology does not fit comfortably as a branch of pure logic, and looks rather different from the practice of most sciences, then perhaps we ought to leave its classification for the moment in order to analyse an example of what it actually does in an effort to see what sort of activities it carries out. I suggest we look at some burials, as this is standard archaeological practice.

We have to hand the results, from published excavation reports, of several cemeteries of roughly the same period (as judged by radio-carbon determinations) and roughly the same type, as judged by the size and distribution of grave, the treatment of the corpse, and the material placed in the grave. The reports tell us that all the skeletons have been examined and divided into male and female – the reports say this, I stress; as always we should be ready to question it – and a small proportion of graves are described as containing either X-find or Y-find (the reports say spears and shields), or Z-finds (the reports say necklaces). Further, the reports suggest a correspondence between X-finds and Y-finds with graves said to be those of men, and Z-finds with graves of women. The next inference is that objects put in the graves were used by the person in the grave during life, so that the *men* with *spears* and *shields* were *warriors* and the more or less equal number of *women* with *necklaces* were their *wives*. One report might have gone even further into the realm of abstraction, which in the last sentence is shown by italicising the interpretative words, by suggesting that the proportion of warrior graves in the total number of men in the cemetery gives the proportion of warriors in that society and, since all other graves were totally unfurnished, warriors form the aristocracy. In this further development of ideas italics would need to be multiple to give any idea of the distance the interpretation has moved from the reported observations in the excavation report.

What concerns me here is not the example itself, the warrior, the spear or the aristocrat, nor the fact that the excavation reports have gone into interpretation, but the process which has gone on and the nature of the different statements made. Obvious points at which the example could be castigated are the division of skeletons into male and female; how was this done, is it a valid distinction, what is its reliability, or repeatability? Or the very mention of spears, which is why I introduce X-finds in case I want to avoid any interpretation at all. Yet more exception might be taken to that last sentence than to anything quoted from the excavation reports, for it suggests that I can set words down on paper on the subject of an excavation and the resulting finds, without interpreting.

I am dissuaded by a philosophical advisor from starting to niggle at too fundamental a level. To some people observation and measurement are already heavily theory laden activities, way divorced from any claim for objectivity – whatever that might be. Thus in deciding to measure our grave outlines, and the length of X-finds and the length and breadth of Y-finds, and examining features of the skeletons, we are foisting on our material, which we have set before us by highly interpretative processes of selection and excavation, a whole host of preconceptions. First, that measurement of length or breadth is a valuable activity; then, that centimetres rather than inches are the correct units to use, and then, worst of all, deciding to take one or two measurements and play with those, when there is an infinity of measurements which could be taken and played with. All these actions will be dictated, or at least

influenced by our past and therefore the culture in which we grew up; they might thus have no relevance to the culture that we have excavated. So, if X-finds are called spears the obvious measurement for us to take is one of length because of stories we read as children about the wicked long spears used by the natives in the Zulu wars. Even if X-finds were spears, the users might well have regarded the length as unimportant, but the tree, not the species of wood, but the actual tree from which the shaft was cut, as the prime fact about it. Even if the shaft remained, to be identified to species, we would still never classify the spears correctly, that is in the same way as their makers and users, because there were ash and beech in the sacred grove, and elsewhere, and the vital thing was whether the shaft was cut from THE oak or THE ash of the sacred grove.

With this barrage of objection, which I find totally compelling, how can I continue to talk about archaeological facts, objective statements and measurements, when I have agreed that what I do to the material might be irrelevant in the eyes of the society that I say I am studying, and can therefore mean nothing? Here we reach the depth of meaninglessness. I maintain that I can extract material from areas of the ground and publish them in terms as near objectivity as makes no difference. This is a fantasy, and not, to me, an interesting one, but I have to put it in to show that I really do mean what I say and that I really do believe that a near approach to objectivity is possible.

My area of extraction – please notice that I do not say excavation – will be selected from Ministry of Housing air photographs taken in 1947 for totally non-archaeological reasons, by a scanner directed to reject all areas of buildings and to concentrate on open fields. It will select twenty points at which there is, in the area scanned, strong distinction of light and dark tone. Mechanical shovels, grabs and other equipment will be sent to the site which will be grubbed out to a depth of x feet, or metres, the spoil will be taken to a processing plant where it will be fed into a series of sieves. From each sieve, the caught material will be passed by conveyor belt below a strong electro-magnet which will pick up all ferro-magnetic objects, thus probably mostly iron, and these objects of ferro-magnetic nature and groups of dimensions can be counted and weighed without the touch of a human hand. We can then compare the iron objects, by frequency and weight, in the different size categories imposed by the sieves from the twenty sites. This seems to me to come very close to objectivity and to incorporate very few culturally interpretative judgements. I chose the sieve sizes because that was what the gravel company could provide; I chose iron objects because there is a way of picking them out by totally physical and non-interpretative techniques, and I chose my sites purely on the effect of light and dark on a scanner.

I presume there will be two reactions to this. One will admit that the process is as near as makes no difference un-interpretative, or objective, but totally pointless. The other will continue to niggle, even at the judgement 'as near as makes no difference'. With the latter I give up since they seem to be more intent, like a debating club, on the argument than on the possible result. The charge of pointlessness I accept whole-heartedly, because this is a vital part of what I want to dissect out. The totally objective statement is at the same time almost a re-statement of the obvious, and of a high degree of certainty. The result from my twenty sites will be unassailable, though meaningless to archaeologists. If the sieves are free from faults,

and the electro-magnet is working, we shall have a true and incontrovertible statement of the iron objects which were grubbed up from those twenty sites. Even those who niggle at the absolute objectivity of this process might, I hope, agree, that this example must be judged an extreme case of attempted objectivity in excavation. But since this is of no use to anyone whatsoever why should I take time and effort to insist on it and describe it? Because I want to establish that I do mean what I say when I talk about attempting objectivity in archaeology.

If we can take that now as settled we can go on to 'use', purpose, subjectivity, interpretation and all the rest. That example is useless to the normal archaeologist because not only *does* it not 'mean' anything, but from the way in which it was done, it is totally debarred from ever meaning anything. Does this not also mean that since it was not done interpretatively, and has not been reported interpretatively, it cannot ever be interpreted? In other words, there is a major difference between an interpretative approach and an objective approach? It will now be obvious why I had to go through my gravel extraction before I could make this simple statement with any hope of getting it accepted. Without the gravel extraction some people would refuse to believe in an objective approach; with the gravel extraction there is a faint gleam of hope that they might accept its possibility, so long as that is joined with an acceptance of its total futility. Now, I find the reason why some people would fail to believe in the possibility of an objective approach rather interesting. They were giving me the credit for not wanting to do something pointless (in their eyes) and wanting me to be something like themselves in humanity rather than judging me as a robot. Thus they might have worked out what an objective process would be, but since they know that I like to join in the general process of interpreting the past, and that I am aware of the rules necessary to be able to do it (stratigraphy and so on), I did not mean a really objective process, which would be useless, but a useful process which they rightly saw could not be objective. If I have managed to demonstrate the possibility of doing something objective, certain, and pointless, I am quite happy not to do it. But its existence as at least one pole of a continuum from objectivity to subjectivity, is now a vital point in my argument.

My gravel extraction would be pointless, would not really be archaeology, because I did not start the extraction with archaeological motives. I presume that means with the motive of understanding something about the past in terms of people, events, systems or whatever. I do not know what date these sites were and, since only iron was saved, I shall probably never know. Even if the iron could be dated there is no separation between modern artefacts such as nails and tobacco tins, and Iron Age brooches. Because of the way the material was extracted there is no possibility of understanding or interpretation. Here an interesting possibility arises. If the process had been much less clumsy, much more refined, but still in mechanical terms rather than in interpretative terms, the possibility of interpretation gradually rises, while I can see no reason for saying that the objectivity decreases. I think therefore that it would be possible to present an archaeologist with a set of material, obtained by totally non-archaeological means, thus, to me, virtually objectively, and yet give him or her the possibility of interpreting the material and making something useful out of it. I believe in the possibility of producing the material objectively and then studying it interpretatively. I will accept that this will often be inefficient in that material will obviously be better material for interpretation if it

is dug up with that interpretation in mind. But we have already come back into the main stream of archaeological argument with the problems of forcing a pre-determined theory on to a site or on to a set of material, rather than allowing an open minded approach. I would summarise this section by repeating that there seems to be a sliding scale where the more obvious the statement, or the more something is merely a statement of the obvious, the higher degree of certainty it has, and the more interpretative a statement, the lower its degree of certainty.

We might, for the moment, suggest that excavation, and the presentation of results of excavation, ought to be made with the maximum objectivity and the minimum subjectivity; then there ought to be a passage of interpretation which all should recognise is subjective and therefore of far less certainty than the presentation. The vital point seems to me to be the decision of which category any paragraph of a report falls into. The grave plan, admitting that the colour change which set someone on to clearing out the disturbance of the subsoil, was noted subjectively by a person rather than objectively by a colourimeter, ought to attempt objectivity. Thus there should be a mention of where edges were indistinct, a mention of what the difference between the fill and the surrounding material was, or was taken to be, and a rough guide ought to be available on the correspondence between the record that was made and the hole in the ground. In the same way the grave plan ought not to differ by more than a certain amount when drawn by 10 different people, of different cultural backgrounds but with basic archaeological training, at different times of day, by natural and by artificial light, and so on, and so on. In other words there ought to be accuracy in the reporting of what was where in terms of shape and size, and I believe this to be possible within certain investigable limits.

Once the things in the grave have been removed and are to be described then, again, there should be little disagreement over what constitutes something in the fill of the grave and the grave matrix or outline itself. These things should be described and drawn in as objective a fashion as possible. If this involves calling one type of object X-find so as not to prejudge the issue of whether it *is*, or even more important, *was* a spear, then so be it. It might be noted that X-finds were generally 2.2 times the maximum dimension of Y-finds – in interpretative language, that spears were 2.2 times the length of shields. And when all this has been put down on paper I have the feeling, which is supported by one school of philosophy, that I have established valid observations which have a high degree of certainty. For the moment I do not suggest that these observations mean anything beyond a statement of the obvious; the X-find in feature 52 was 7.6 units long in its maximum dimension. If I have described what I have done in describing the finds, and have left a suitable measuring instrument in the museum with the finds, then every person who followed me ought to get the same results. This will not mean anything at all and I have to refer again to my gravel extraction to show that I really do mean this, other than that all observers following the same rules of procedure have come up with the same observations. In this line of reasoning we might attain total, formal certainty. If, for example we performed a chemical analysis on one of the finds and obtained results of 80% copper $\pm 1\%$ and 15% tin $\pm 1\%$ there is formal certainty that the other elements present cannot be more than 7%. The certainty comes from the definition of percentage for, if those two values of metals are correct the only way we can reach a different result is by faulting the whole idea of percentage. It means nothing: for instance it does not tell us the weight of other

elements, only the percentage, and that is already, in a sense stated in the two other percentages. The more obvious the statement, the less meaning and the greater certainty.

If we return to our cemetery we might start by interpreting skeletons with one set of material characteristics as male. The narrow sciatic notches, sharp orbital edges, and smaller frontal sinuses are held to belong to skeletons which in life belonged to men. This is known to be an interpretation which can be inaccurate. Some people would rate it less accurate than the deduction that people buried with spears and shields were warriors in life; in fact, some cemetery reports might well start off sexing people first by grave-goods and only secondarily by skeletal characteristics. This is the sort of disreputable nonsense which makes archaeology the laughing stock of any scientific gathering, for the two deductions are utterly different in quality. Whereas the attribution of sex to a skeleton can be given an estimate of error from further observations in modern material the donation of the rank of warrior to a skeleton buried with a spear and shield can never rise above the level of present preferred explanation and cannot be given any indicating grade of certainty. It does not mean that such an interpretation is wrong, that it is unlikely, or that the process of interpreting is reprehensible; it is simply a totally different class of activity from numerical observation and manipulation, or of fitting observations to a verifiable background.

In the case of sciatic notches all the 200 skeletons in the cemetery may be measured. The results may be expressed in a table and it is likely that there will be a bi-modal or two peak distribution. Put crudely, there will be two sorts of sciatic notch with a degree of overlap between them. Extreme values will obviously belong to one class or the other; values around the two peaks will belong to one or the other, but values in the middle between the two peaks may belong to either group. This is a general finding in skeletal material. The values are not absolute but relative – wider notches and narrower notches, but the European narrow notch may have the same angle as the Asiatic wide notch. Sciatic notches can be measured, in theory from X-rays taken on living people (though this must remain unpopular because of X-ray damage in the pelvic area), and they can be measured with ease on numbers of recently deceased bodies. The same bi-modal distribution may be observed every time such an experiment is done, and every time the narrower notch is seen to belong to the skeletons who were men when they were alive, and the wider notch to women. The range of overlap can be found by direct observation and the very small chance can be observed that the narrowest notch in some group does in fact belong to a woman. I am encouraged by obstetric advisors to think that the group with the narrower sciatic notch have probably never indulged in child-bearing.

I do not see that a similar process can be constructed for using the word ‘warrior’. The rationale behind the appellation man, and the investigation of sciatic notches, is based on a functional difference between male and female which seems unlikely ever to have altered; man or woman is therefore used in this context purely on the basis of reproduction. I can see no such connection between the concept in my mind of a warrior and the evidence of a burial. However many burials I observed in the modern world there is no necessary (note the word is being used almost technically) connection between a man fighting with a weapon in battle and a given mode of burial. I might observe a connection in 99% of the cases that I observed in a

long life of burial watching, but the society from which my burial-to-be-interpreted came was not, by definition, one of the living societies that I observed, so there can be no logical extension of a law compiled from the living to the burial of the past dead. The fact that I have observed a 99% correlation in the modern world might give me cause to interpret the burials I excavated in a certain way, and that interpretation might reach the status of presently preferred explanation, but that cannot be given a probability value of being correct or a judgement of its certainty. It is a completely different animal from the limit of error made on a numerical observation.

If we go on to consider the link between the numbers of men buried with spear and shield and the number of women buried with necklaces, the fact that they can be seen to be associated in space, and the fact that these are the only furnished graves in the cemetery, then we might arrive at a suggestion that the warriors were the top class of the community. A statement such as this is probably too complex to be tested empirically either from direct observation or from written sources, for there are so many elements which have to be defined along the way that the resulting uncertainty from each one would leave the 'test' in a complete muddle. What do we define as class, what is a warrior, why do women get buried with necklaces, how do societies rate warriors, and so on. It remains a perfectly reasonable interpretation, it avoids any obvious contradiction, but it is no longer in the privileged position of 'clearly preferable interpretation'. Reference might be made to a modern society in which only the fighting soldier might be associated with his arms while the commanders rose above such things. The same could be suggested for the Roman Empire (though weapon graves are rare and probably un-Roman) in which it would only be the fighting man who would have arms while the upper classes even in battle would have less to do with material equipment. The commanders would return to Rome to be buried in the manner of their ancestors while the foot soldiers or cavalry might be buried in association with equipment. There are plenty of other explanations for only a small group within a cemetery being buried with arms, and simple necklaces may be more evidence of beads for the natives or shopping at Woolworths than a place in high society.

We might go one stage further and suggest that in our cemetery, which consists largely of unfurnished graves and is radio-carbon dated to the years around AD 400, a small number of spear and shield graves and necklace graves show the presence not of Romans but of Saxons. I have dealt with some of the problems involved in such an argument elsewhere (Reece 1982) and I do not want to go into it all in detail again here. Perhaps I can say in brief that research by material observation might show the burial rite involved to be common in Saxony and absent in Britain before AD 400, in which case one might properly call the burial rite Saxon with a clear geographical and temporal intention (i.e. of Saxony), but this would say nothing about the ethnic or social or political origin of the people using this rite. It might be that further material search would find blood grouping in Saxony consisting of 50% P and 50% Q with an absence of R; while in Britain in the 4th century the groupings were 10% P and 10% Q with 80% R. If the unfurnished graves approximated to the British pattern and the furnished graves to the pattern from Saxony it would be a reasonable inference that some population movement from Saxony to Britain had taken place.

We therefore have levels of interpretation and levels of certainty, but those levels of certainty must not be confused with the reasonableness of the interpretations; a level of certainty must not be rated more likely to be true than a reasonable or preferred explanation. It is only a currently fashionable snobbery to prefer an explanation which can be demonstrated to have a 60% probability of being so while leaving a feeling in the mind that it is human nonsense, to an explanation which feels right on the human level but cannot be expressed in such a way as to receive numerical manipulation, estimate of error or any value of probability at all.

Our spears and shields can be recorded numerically, classified and subdivided by the methods, if necessary of botanical numerical taxonomy, and compared by methods developed in astrophysics. If each of these methods is an integral part of mathematics these processes will have the reliability of mathematics to state and manipulate the obvious and there need be no human intervention or human feed-back. There may be statement without either deduction or interpretation. We may measure our skeletons, perform the same measurements on a modern population and, from these observations, show how any given skeleton related to a modern counterpart. From this numerical demonstration we may deduce that the skeleton in question is, more likely, by 9 to 1, to have belonged to a man. So much is biologically determined; to go further and say that this skeleton is more likely to have done the washing up than the cooking is to introduce a totally cultural anachronism of no validity whatsoever. Man means no more in this context than 'carrier of X and Y chromosomes' as opposed to woman which would mean only 'carrier of X chromosomes'. Even at this level we are on the edge of the chasm which separates that which can be demonstrated by material research and that which is reasonable. Suggestion that our spear and shield burials are warriors is eminently reasonable and within the possibility of verification through material search and comparison. This will not amount to proof but may attain the status of preferred explanation supported by 98% of all known examples. Warriors as a Top Class must be relegated to the position of the chosen idea of the commentator for it can easily be challenged by analogy. Warriors as Saxons, as opposed to arrivals from Saxony with a distinctive burial rite and blood grouping, enters a much wider field of dispute ringed with barbarian rape and pillage as opposed to Roman tranquillity. The field has opened so widely from numerical demonstration that I want a new category of explanation and I suggest the category – myth (see Reece, forthcoming).

Why should a simple preferred explanation be nearly related to a mathematical demonstration, a form of compliment, but a complex preferred explanation be damned as a myth? To many people there is the suggestion that the first is as near as possible true while the second is just an invented piece of nonsense. This would be totally to misunderstand my use of the word myth. I am introducing it in the sense of the theologian who is faced with a past event which, taken at face value, is surprising in a material sense, in fact rather unlikely in a restricted physical sense, yet an event which has been endowed with a remarkable amount of meaning in a non-physical sense. An example which has not raised particular passions is the visit of the Three Magi to Bethlehem. Liberal theologians may find many reasons for not insisting on belief in a totally step by step account of this visit, yet few, if any, would wish to eliminate it from a consideration of what Christianity was all about. As a part of a whole

complex of events and ideas it has an importance rather greater than its likelihood of being materially true. A direct report of the visit could be well summarised in five thousand words; the implications of the Epiphany could spark off a deep three volume work of the most complex theology. The simple meaning can be easily transferred from mind to mind because it is factual; the complex meaning is embedded in each believer and can only be transmitted at length and with difficulty because it is conceptual. This seems to me to be a very good description of the use of the terms Roman and Saxon.

It is no accident that the mathematical way of dealing with things is at one extreme to be applied to repeatable observations of the material world so that the human being scarcely needs to enter in to the process; that myth is also at the opposite extreme in which the interacting elements of observation, deduction, suggestion, explanation and interpretation are so complex that a large element of the person attempting such an activity comes into the final result. The subjective element in the first process is minimal; in the second I suspect that the whole process has to happen through the worker's mind and imagination and is therefore very strongly subjective. While there was hope that the earlier stages could reach a preferred explanation in place of some sort of certainty, which is unattainable once we have moved from observation to deduction or interpretation, I am unsure whether a preferred myth has a greater validity than a novel myth, or an unpopular myth. In de-mythologized language: a moderately simple interpretation of observations which sticks fairly closely to the observations from which it started is likely to attract adherents and users according to the neatness of the explanation, its correspondence to known observations, and its simplicity. There is hope that the consensus which surrounds a preferred explanation is empirical, fairly objective and related to the archaeological matter in hand. When an interlocking series of observations are bundled into one highly complex interpretation which only uses the observations as starting points for personal construction, then the consensus which surrounds a highly complex interpretation, such as the explanation of the de-Romanization of Britain in terms of Romans and Saxons, is not so much a convergence of empirical judgements but a quagmire of personal hang-ups. The example given, of Britain between AD 300 and AD 700, strays quickly into the conflicts of religion and atheism, defence and pacifism, right and left wing politics and anarchy, progress in the sense of organization and the simple life, as well as all sorts of conflicts in economics, medicine, climate, and a host of other studies. I do not believe that each person who takes an interest in such a complex series of events and interwoven set of explanations will think out each step in an empirical manner, I think he or she will apply their inbuilt parcel of beliefs which makes the whole subject of such explanations or interpretations one of myths.

How then at the furthest end of the spectrum of explanation are such complex ideas or myths, judged? To take refuge in a near tautology, myths are useful when they help us. They help us to understand intricate details of the way we live and the way man in general behaves. They feed in not so much to our rational selves as our emotional selves, and this is the first point at which our archaeological world has made any contact at all with the world in general which professes a great interest in our subject. I must make it absolutely plain here that I am not suggesting that the past ranges from observation, which is the diet of the archaeologist, to myth, which is the entertainment of the masses. Archaeologists love myths just as much as

the general public and invest far more emotional capital in them. The other activity which consists of capturing human activity and emotions and putting them forward for the entertainment and judgement of others is the writing of novels. I suggest that archaeological explanations which are so intricate and far-reaching as to deserve the title or warning signal, myths should be judged in the same way as novels.

My philosophical adviser saw nothing helpful in the ideas of either myths or novels. He pointed out the difficulty that the novelist is not constrained at any point by historical facts and is therefore free to write complete fantasy whereas we might assume that even at this complex level of interpretation the good archaeologist would try to make his myths at least consistent with known observations, and should be willing to adapt his myth to fit new research. I shifted the subject fairly quickly because it was not congenial, but if my mind had been working quickly enough and I had wished to push further I would have suggested what while the novelist may be totally divorced from accepted facts from the past and may even incorporate direct contradictions to 'established truth' – for example that Napoleon killed Wellington and won the battle of Waterloo – he is still constrained by the nature of human beings if he is to communicate with us and we are to find his work appealing and attractive. Given that the archaeologist has the added difficulty of staying within the known observations on the past he shares, in my estimate, the problem of the novelist in fashioning complex interpretations of the past which are true to human nature. Here I may soften an earlier judgement by which I felt that a consensus on myth was no more than a tangle of warped emotions. The judgement on a novel in the years after its publication can often be seen to be widely different from the judgement which settles down as a consensus say fifty years later. Where novels are more than fifty years old I find myself in general agreement with the consensus, often to my surprise. Until I was 36 I knew that the novels of Dickens were highly esteemed but I knew, in the absence of direct experience, that this was a typical conspiracy of English literature specialists to defend a thriving industry. Then *The Tale of Two Cities* was the only unread book in the house and I started it just to see. Within four years I had come to the reluctant conclusion that a reading of the complete works brought me into line with critical opinion. I mention this to suggest the way that such consensus develops and to say why I now have some faith in it. But if there can be a genuine consensus on the excellence of a novel I think I have to admit the possibility of a genuine consensus on the excellence of a myth. This presumably means that when a series of complex explanations of series of observations on material from the past, or archaeological myths, have been carefully constructed and put to the test of a century or so, a few will grow out of the general undergrowth and might act as signposts to understanding. Just as the current novel, such as *The Name of the Rose*, creates great critical noise because it massages the neuroses of the decade, so a complex interpretation will appeal to people at one particular point in their own lives, or their societies' misfortunes. When these temporary aids to success have passed, then the great novel will remain attractive and influential in spite of its distillation of the neurosis of the age in which it was written and the complex explanation may remain attractive when society has weathered the storm which provoked its construction. Or the reverse may happen and the products will be seen as no more than superficial reactions to the times.

At one end of our scale there will be a measure of certainty that the observations are correct, producing the likely question 'What are you going to do with them?' At the other end of the scale will be convictions that the complex explanation is wrong and needs to be extirpated before it does damage to tender minds. The reverse is unlikely simply because the observations ought to be verifiable, so that there is little point in disagreeing about the correctness of observations for they ought to be continually verifiable; this is the antidote to any disagreement. Strong feeling may be aroused 'a very slapdash piece of work with five out of twelve observations not accurate to within 2 per cent' but those feelings can be sorted out and defused. Whether the myth is good or bad will, unfortunately, be a matter, not of certainty but conviction.

I suggested that a good archaeological myth-maker ought to take into account all available observations on material from the past relevant to his period and subject and make his myth consistent with them. This, though time consuming, is possible. When new observations come to light he ought to change his myth to take account of the discoveries. This the myth-maker might be able to do, but he will find it very difficult indeed to drag his professional colleagues with him and he will probably have no effect at all on his public consumers. His first attempt at myth will have to be firmly in concert with some aspect of current thought for the public to take to it in the first place. While the mythographer may be more attached to his archaeological material than this particular myth, and so able to change, his public will have only the final myth to which to attach and they will change much more slowly, much more reluctantly, or not at all.

The point of contact with the public at the most complex, and at the same time, the least easily argued end of the archaeological spectrum poses a problem to those who would communicate their work to the public that pays them. They are usually paid as expert archaeologists and are, presumably, expected to perform as such and not as entertainers. In the short term the paying society may prefer the entertainer, but when in later years he turns out to have been an illusionist, the job of any successor will presumably be more difficult – if job there still is. This is an issue which needs more careful discussion than it can receive here as an offshoot of the main argument.

My conclusions will perhaps surprise no one but myself and though they may find adherents each archaeologist will go his own way. The best that I can hope for is that some of the ideas may act as sounding boards on which each person interested in the past can construct a theoretical theme of his own.

Acknowledgements

Several people have helped me to reduce my confusions of mind. It would be unfair to name my adviser on philosophy, but he gave much needed support at a vital stage; my obstetric advisors retain their usual professional anonymity. Ian Hodder would probably prefer not to be named at all because although I have tried to counter some of his valid objections I know he will still regard this as uninformed and backward-looking; this is a spur to

me to become better informed and forward-looking. Steve Roskams, Martin Millett and Jeremy Evans have tried to sort me out; that they have failed should not reflect badly on them.

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The Computer Takes to the Field

by B. A. P. ALVEY

Introduction

Five years ago, barely four or five regional archaeological units in Britain possessed or used microcomputers in the field and fewer still were used by British archaeologists for excavations overseas. Since that time, as their price has fallen, computers have been used much more widely by archaeologists. New types of microcomputer have appeared on the market which are more resilient and portable and which are much more suited to the needs of the archaeologist. Soon the computer will become as much a part of the archaeologist's tool kit as the trowel. This paper attempts to outline the changing face of archaeology as the discipline absorbs the new facilities offered by this modern technology, and outlines the potential for its use in the field. In addition, the author will take two case studies of which he has had personal experience, from the sites of Ban Don Ta Phet in Thailand, and Salango, Manabi, Ecuador.

Which computer?

As prices fall, so more and more computers become available for use to the archaeologist. As more and more appear, so the confusion increases as to which model to choose. This section will not attempt to provide specific examples for computer purchases, but will try to set out the type of computer that might best be suited for particular aspects of use in the field.

The portable computer

The smallest type of truly portable computer is a very small and convenient range of models which are very light and possess their own power source. These models can be carried out into the field in the larger pockets of a waterproof and used in all weathers with impunity. The author has had some experience of these whilst working in York, where an Epson PX-8 was used with some success to record data in a damp, dusty and freezing church. The main advantages of this type of computer was its portability and resilience in adverse conditions: the system supported a portable version of Dbase II which enabled the user to relate the data stored to a host of associated files when the information from the Epson had been downloaded to a larger computer. Small command files made the system user friendly and enabled even the least computer literate to store data with ease. Since Dbase II is one of the more common and flexible databases sold commercially and popular amongst archaeologists, data captured by these smaller computers are able to be accessed easily into the databases of a number of institutions.

The disadvantage of such a computer is in the amount of storage space it carries: the Epson carried about 50k (20 pages of script) in RAM and allowed another 100k (40 pages) to

be stored on a tape, although this latter form is tedious to write to and from. Other computers, however, have recently been released on the market that can handle much more data than this. Secondly, the amount of energy held in the batteries for this kind of machine is limited; typically it lasts no more than a day of continuous use before recharging is necessary. Also, whilst this sort of computer is quite hardy for the archaeologist, it is vulnerable to the odd knock, where data left in RAM can occasionally be lost. On the whole, this sort of computer is best suited to the archaeologist who requires complete portability for his machine, yet has access to a larger computer and an electricity supply, preferably on a daily basis, for downloading information to the larger machine and recharging the batteries. This kind of machine is mainly for use of data capture only: data manipulation is severely restricted. Used in isolation, data capture too is limited; for large quantities of data this type of computer should always be used in tandem with a larger, and probably less portable, machine for downloading data for analysis.

The semi-portable computer

For such applications as excavations the archaeologist may choose from a number of semi-portable, lap-held or 'luggable' computers which have been designed with mobility in mind, as well as for the office desk. These range in price from about \$400 to about \$3,100 and their use to archaeology varies accordingly. The recent (and most expensive) additions to this market, such as the Toshiba 3100 and the Tava Flyer, etc., now offer almost complete portability coupled with a large amount of storage space. A more detailed assessment of the use of one of the models in the lower price bracket (the Kaypro 4) appears below, but some of the advantages and disadvantages of these products are given here. Almost all the models that fall under this category can be carried using one arm, although with some discomfort when hiking one of the more heavy models around. All can be taken as hand luggage on planes, parked between seats on trains, or carried in the boot of a car. The ruggedness of each computer may vary and, on the whole, more care needs to be taken with computers with hard discs than those with floppy disc drives – but for the most part these computers are designed for and can stand up to a certain amount of ill treatment, such as the odd knock or shower of rain, or exposure to dust.

This type of model generally requires an electricity supply, although some can be run on batteries which can increase the weight of the computer and run the machine for a very limited amount of time before recharging is necessary. Alternatively, such computers can use electricity from generators if no mains supply is available.

On the whole the usefulness of such machines is reflected in the price. The machines at the lower end of the market generally read data from and store data to floppy discs that can hold typically between 200–800k (80–300 pages) of data on each disc. This allows a certain degree of manipulation on site and, indeed, desktop versions of these machines often form at present the mainstay for analysis in post-excavation. For larger applications, however, the constant changing of discs to and from the machine can prove to be tedious and, to the inexperienced user, confusing. For data capture, however, and on site word processing, these machines are often adequate for the archaeologist for the storage of his written data.

For more complex sites or more demanding applications computers supporting hard disc systems are becoming available in the higher price range of the portable market. Often these have a 10M (4000 pages) hard disc for storage, together with a single floppy disc drive. The most expensive of these also have graphic capabilities. This system allows quite complicated analyses to be accomplished on site and large numbers of data to be manipulated in the excavation environment to aid the archaeologist in his strategy.

The desktop micros

For semi-permanent, or more extensive excavations, some mention should be made of the desktop microcomputers of large capacity that are, at the time of writing, currently available to the archaeologist for more specialised applications. For some excavations the further expense of portability is of small consideration for the archaeologist: the ease of adding peripherals (e.g. digitisers and plotters, printers, etc.) and extra storage space is more important.

Here the choice is alarming and with relatively small amounts of money major excavations are able to accomplish quite complex tasks on site, drastically reducing the timescale between excavation and publication. Since the applications are so varied when dealing with machines of this category, the reader is referred to the author's own personal experience of such a system in the section below.

Excavation or post-excavation?

The main application for the computer in an excavation context is primarily for data capture and, to a lesser extent, data manipulation. Thus the database is of utmost importance. In this context commercial databases are available, indeed are often supplied with the computer, which are more than adequate for archaeological applications. One main criticism of these systems has been that they provide far more applications than is needed for use in archaeology, a sort of 'overkill'. This has resulted in a number of archaeological alternatives designed specifically for site use. The author's experience of these has been that they have not been sufficiently flexible for ease of use on a number of sites. The effect in these cases has been that less data is stored in the computer archive than had been accomplished using traditional methods; in the author's view, a retrogressive step. At the time of writing it seems that the commercial database, allowing greater flexibility and more sharing of data, coupled with simply constructed interfaces for the archaeologist, is still the most practical solution.

Once the data are stored in the computer the archaeologist is able to rearrange aspects of them in a report format and in various combinations obtain fair copy for consultation, display, or to aid in site strategy. The database itself also forms the basis for the site archive whose construction traditionally forms the bulk of post-excavation work. Additionally, packages are available which can automatically provide matrices showing the relationships of all layers on site, a task which until recently fell to the analyst in post-excavation.

One of the benefits of the onset of computer usage in on site excavation has been the speed at which the data can be checked. This has proved most useful in correcting errors in

excavation, e.g. in stratigraphy, etc., which can be confronted whilst the excavation is still in progress. Often problems such as these can only be solved during excavation which, using traditional methods of recording, appear all too often six months after the excavation has ended.

It seems therefore that the traditional line separating excavation and post-excavation work has become blurred and that more and more of what was previously considered post-excavation work can now be established in the site context. This may soon influence the way in which archaeological units are internally organised, with less time spent on simple record archive and more attention given to interpretation and research.

The computer, therefore, is having some profound effects on the way excavations are organised and a different expertise is being asked of excavation staff. At a recent conference one member complained that his supervisors were under extra strain from the increased paperwork and extra information they were receiving on site; they felt that they held a more administrative position than under a traditional system. The role of the supervisor is indeed changing: extra man hours must be spent in typing information into the computer and checking for errors. That work input must be co-ordinated with on site work; much of the error checking should be the responsibility of the supervisor or the director; the supervisor is also responsible for the correction of errors that would otherwise not have been discovered until some time after the excavation; thus the excavation staff are inevitably drawn and partake in the post-excavation process.

Some reorientation is therefore necessary: staff on site must be able to cope with the fast response to site recording errors; they should have rudimentary computer skills and know what questions to ask of the computer. As computers become a common part of site recording, so more money should be made available for the training of archaeologists in computer usage. In the same way that companies consider formal training for their staff an essential part of improving office efficiency, so archaeologists should be trained to maximise the potential of the computer on site.

In the field: an example from Ban Don Ta Phet, Thailand

The conditions faced at Ban Don Ta Phet in Thailand seem at first sight very difficult for reliable computing. The site itself lies in the grounds of the local school on the western edge of the village, some 8 km north of Phanom Tuan and 1.5 km west of route 124 between Kanchanaburi and U Thong in West Central Thailand. The village itself is situated on a low terrace rising above swampy ground, which is cultivated predominantly for rice and sugar cane.

The site was first discovered by schoolchildren in September 1975, when post-holes were being dug for a new boundary fence for the school. Excavations were undertaken by the Thai Fine Arts Department between November 1975 and May 1976. Some 84 burials were uncovered during this period, rich in iron tools and bronze vessels, with evidence of ornaments and jewellery fashioned from bronze, glass, bone, shell and semi-precious stones. Publications of that excavation have appeared as a museum booklet written in 1976 by Mr Chin You-di for an exhibition of the finds from Ban Don Ta Phet at the National Museum, Bangkok, and as

undergraduate dissertations by students of the Faculty of Archaeology, Silpakorn University, and the Institute of Archaeology, London. Preliminary results of one of these projects have been published by Warangkhana Rajpitak and Seeley (1979).

In the light of the preliminary results of the first excavation a joint project was agreed between the Institute of Archaeology, London, and the Fine Arts Department of Thailand between November 1980 and January 1981. Chin You-di felt that the cemetery should be attributed to a settled farming community of the Late Iron Age, dating to a period before the appearance of centralised kingdoms in the mid-first millennium AD, organised according to the religious and political models introduced from India. The site, however, provided evidence of contact both with India and the Malay Peninsula and seemed to have the potential for throwing new light on a very important, but yet little understood, period in the history of Thailand. The justification for continuing the 1976 project, in the context of current problems in Thai archaeology, is set out at some length elsewhere (Glover, 1981).

The second season of excavations took place between December and March of 1984–5, after the initial excavations of 1980–81 under the direction of Dr Ian Glover of the Institute of Archaeology, London University.

The computer used was a Kaypro 4, an 8 bit 64k microcomputer which was kindly donated to the project by Kaypro Inc. (USA) as an aid to the research programme. Whilst this computer is somewhat dated now in terms of portable computer technology, at the time it was one of the leading computers in the market, having a reputation for ruggedness and reliability. The machine has two disc drives running double sided, double density 5.25 in floppy discs, and these, together with a 9-in monochrome screen are housed in a sturdy metal casing into which the keyboard also fits to provide a resilient and portable machine. The machine itself weighed some 13 kg, and some of the computer's rigidity had been attained at some expense of portability as it seemed bulky and heavy when taken on long journeys. However, the machine was made to endure some adverse conditions, and came through with flying colours.

The computer was used in the houses rented from the villagers, some two minutes from the site itself, since the school was unable to provide an adequate power supply. The houses themselves were of traditional construction, which often became thick with a pink dust raised by the buffaloes, goats, pickups and tractors that passed by on their way to the paddyfields. Furthermore, the excavation took place in the dry season and spiral dust clouds whipped up by the warm winds could be seen to pass over the village.

Some problems were anticipated from the dust, which was expected to affect the floppy discs, but none were encountered due to the precautions taken: the disk box was sealed and wrapped in a further cover of polythene when it was not in use with a bag of silica gel to protect the discs from the frequent changes in humidity. The computer too had a dust cover, but as it was in use for most of the day it had to operate in spite of the conditions. We cleaned the heads frequently and – although still now some dust can be heard when the keyboard is shaken – it has always operated faultlessly.

The dust was not the only problem: during the day it became very hot and there was sudden and fluctuating humidity. Also the electricity supply in the area was somewhat less than reliable and without an earth. An earth of our own was constructed and the computer was

linked to an 'Isoreg' voltage regulator, lent to the project by Precitek Co. of Bangkok. The regulator ironed out all surges, spikes, glitches and other irregularities in the power supply; enabling the Kaypro to work in everything less than a total power failure and no data was lost.

The computer made a major contribution to the efficiency of the operations on site. As the archaeological information was uncovered, it was loaded on to the Dbase II database system. Any omissions or errors were located extremely quickly and a more detailed documentation of the site was accomplished. The database files created at this time now provide the core of all post-excavation analysis and publication.

Some 5,000 finds were recovered from 31 protohistoric burials during this excavation. These included bronze bowls, bracelets and anklets, and a wide range of iron artefacts such as arrowheads, spearheads and adzes. Over 600 beads were recovered from the site, made from glass and various semi-precious stones such as carnelian and agate, and a green stone pendant, made from either nephrite or jade, has important marked parallels with other finds in South East Asia. The exact locations of each of the artefacts discovered were recorded and a detailed examination of the more diagnostic pieces were documented in a standardised form and loaded on to the computer in the field.

Since the Kaypro 4 had no hard disc, and had only an 8 bit 64k microprocessor, Dbase II could be used with only limited flexibility. In practical terms the data concerning a particular aspect of excavation (such as context information or bronze objects, etc.) could be stored on a single floppy disc at any one time, and thus manipulation of the data was limited. Also, Dbase II allows only a single database to be accessed at any one time, and so, inevitably, each datatable was stored on its own floppy disc and accessed in separate sessions. This meant that while the data was captured and stored easily enough, complex manipulation of that data proved difficult. Since Ban Don Ta Phet, 16 bit micros using hard discs and such database systems as Dbase III, Paradox, etc., have come on to the market and now allow much greater flexibility on site.

Dbase II is a relational database system in which all data is separated into tables according to the nature of the data. Thus context description, small find location, pottery descriptions, each have their own table. For Ban Don Ta Phet, a site without stratigraphy, the database was centred around a 'Central Index Table' containing the small find number, the context number in which the small find was discovered, the material code and the 3D location of each object discovered on the site. Further tables related to data concerning special find objects of particular interest, each material type having a separate datatable; quantitative details of all pottery fragments were located in a separate databank.

The organisation of the excavation was little affected by the presence of the computer: the usual requirement was for a single member of the team to be occupied each day for data input and checking. The correction of errors often required the presence of a second member of the team and supervision was occasionally required to ensure standardisation of input. The day-to-day running of the excavation was changed only in that standardised forms had to be filled for each aspect of recording. The improved efficiency of recording through error rectification often resulted in a higher standard of recording. Also, questions could be asked of the data to aid finds location and conservation.

Another useful aspect of the computer was its rugged portability. Whilst being somewhat heavier than other comparable micros, it suffered the pot holes, bumps and dust of the Thai roads and tracks without any loss of efficiency. The computer was used in Bangkok, U Thong and Ban Don Ta Phet, and could be carried with ease from place to place and taken on to the plane as hand luggage.

In the field: an example from Salango, Ecuador

Hostile and remote environments need not preclude the use of the desktop microcomputer: the following example, from South America, shows how computers can be installed on a semi-permanent basis next to a site for both data capture and manipulation.

Salango in Ecuador is a permanent excavation in one of the most isolated parts of the country. The site itself is deeply stratified and presents a variety of function and characteristics from many periods in Ecuador's history, including a large number of burials, areas of settlement and large fish middens. The site is set in the grounds of a fish factory next to a tropical beach in the region of Manabi. Again, the climate and location of the site made computer usage most difficult: the main problems here were sand, which very quickly spread into all rooms, heat and humidity, which fluctuated greatly from a cold and damp seafret to a burning sun in minutes.

The installation of computers for this site is only now being undertaken and this paper has been submitted before the author's implementation of the system. They will be set up in an archaeological museum close to the site.

In the light of the experience gained at Ban Don Ta Phet, many precautions have been taken to ensure the safety of the data. Electricity in the area is unreliable and is drawn from a local generator and the mains system. Both these sources are uneven and intermittent and so a further generator is to be installed and all power will be filtered through a voltage regulator. To combat the fluctuating humidity, air conditioning has been implemented in a room set aside for the computers and double doors have been installed against the dust.

Three AT microcomputers, each capable of storing 40 Megabytes of information, are to form the core of the system and peripherals such as printers, plotters and digitisers will be linked into the system for graphic input and output. Information capture and manipulation will take place using a customised application of Dbase III plus over a local area network. Data will be stored on the hard disks and backed up by tape streamer: tapes will be stored in a safe, fireproof environment.

The way forward: future research

The majority of this paper has been given over to the discussion of how best to capture written data. Much research is now taking place in the capture of graphic data in the field environment and the first graphic systems designed specifically with archaeology in mind are now being implemented.

Computers with both high storage capacity and portability are now coming into the price bracket affordable to archaeologists. The pitfalls for the archaeologist in the field lie in the electrical supply for his computer and in the storage of his data. If suitable precautions are

taken, such as those outlined above at Salango and Ban Don Ta Phet, and the necessary training given to staff in basic computer usage, then the computer will soon become the accepted form of capturing both graphic and written data from archaeological field excavations of almost any environment, anywhere in the world.

Acknowledgements

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The Study-Collections of the Institute of Archaeology

by ISOBEL THOMPSON

It is not generally appreciated, even within the Institute itself, that the building houses artefact collections of great geographical and chronological range. One of the Institute's primary functions, as envisaged at its inception, was 'to provide properly classified collections of material, derived wherever possible from scientifically conducted excavations, for the use of the student and the research-worker' (1st *Annual Report*, 1937: 11). The acquisition of study-collections was to be on a par with the building up of the library. This is repeatedly emphasised in the first Annual Report: 'The Study-Collections', pp. 23–8, includes a list of accessions prior to December 1937, a list which largely reflects the main interests of the Institute and the gifts of its supporters, notably the Petrie Palestinian collection and other Near Eastern material, and artefacts from Mortimer Wheeler's excavations at Verulamium, Wheathampstead and Maiden Castle. Most of this Wheeler material was destined to go to museums elsewhere, but not all of it went. It was for many years customary for fieldworkers from the Institute, sympathetic towards it, or in receipt of some financial support, to donate samples of excavated material. The Annual Reports outlined the main additions to the collections (see below).

The objective of the Institute's archaeological teaching was to cover those subjects not taught elsewhere: this is why Near Eastern archaeology received the main emphasis. The Department of Geochronology, headed by Dr F. E. Zeuner, was also intended to fill a major gap in available training, the study of geochronology and 'the relations of early man to his environment'. To this end a parallel collection was started, chiefly of soil samples and stone implements. This department, retitled Environmental Archaeology in 1946, made its own reports on donations, the last appearing in the Annual Report for 1953–54.

The collections briefly outlined

The following pages give a straight extract from the *Annual Reports* and *Bulletins* of the Institute of Archaeology of those acquisitions noted therein, beginning with the description of the collection as it stood in December 1937. New accessions are often described as filling gaps in the teaching collections. During the 1960s interest in 'filling gaps' declined and the entries decrease in number; the last was in 1967. The expansion of the Institute's teaching interests, notably with the admission of undergraduates from 1968, meant a radical change of emphasis from a research Institute with a restricted range of specialisms to a more discrete establish-

ment; moreover, by that date it was far more difficult to acquire 'samples' of excavated material, especially from abroad; indeed, no longer considered quite respectable to do so. The integrity of a site's body of excavated evidence was now more often treated as paramount. This factor provides a good reason for making knowledge of the Institute's collections more widely available. They are in part at least still used for teaching and research purposes but are not, perhaps, advertised enough. When I was studying the pottery of Late Iron Age southern England as a postgraduate student at the Institute, I was surprised to discover some of Wheeler's excavated Wheathampstead and Prae Wood material in the basement, including quite a lot of the published pottery. The Verulamium Museum was also surprised to learn that this was so, having lost sight of the fact that Wheeler kept his finds at the Institute until the new museum was built. It is true that the Palestinian and Near Eastern collections are well known in their field, but it is doubtful whether the range in time and space of the whole body of material in the Institute is at all well known to archaeologists.

The list offered here is culled from the published reports; it describes very briefly only those items considered most important at the time. The references given should be consulted to check for any further information. Only the man-made artefacts added to the Geochronology collection are noted here.

Accessions prior to December 1937 (Annual Report 1, 1937, 24-8)

1. Near East:

The Sir Flinders Petrie Collection of Palestinian pottery, from Tell Jemmi, Tell Fara and Tell Ajjul.
 Samaria, Palestine: Iron Age sherds.
 Mugharet el Tabun, Palestine: stone implements, Palaeolithic.
 Jericho, Palestine: Neo and EBA implements; Sir Charles Marston expedition.
 Chagar Bazar and Brak, Syria: pottery from Mallowan excavations.
 Al Mina and Atchana, Syria: pottery from Sir Leonard Woolley's excavations.
 Ur, Iraq, stone and pottery vessels from Sir Leonard Woolley's excavations.
 Cyprus: EBA to Iron Age pots.
 Egypt: Arab and Chinese jewellery and pottery, loaned by Mr Oliver Myers.

2. Great Britain and Western Europe:

Clacton and Essex coast: S. Hazzledine Warren collection, mostly Palaeolithic.
 Verulamium and Wheathampstead, Herts: Late Iron Age and Roman material from Wheeler's excavations (pending removal of most to the museum at St Albans).
 Maiden Castle, Dorset: Neo, Iron Age, Roman material from Wheeler's excavations 'stored at the Institute for the time being' [some pottery remains, but *not* Site F].
 Smaller collections from Iron Age sites of All Cannings Cross and Casterley Camp, Wilts., Alfred's Castle, Berks., and elsewhere.
 Dunagoil, Bute: vitrified material from hill-fort rampart.
 Kimmeridge Bay, Dorset: pre-Roman and Roman sherds, implements and shale objects from excavations by Dr Henrietta Davies.

Lezoux, France: moulds and pottery from the Gallo-Roman samian kilns, from Felix Oswald.

Salisbury, Gloucs.: excavated material from Iron Age site, lent by G. C. Dunning.

3. General

The Karl Pearson collection of casts of anthropological material, including many of Pleistocene skulls.

4. Geochronology Department (p. 31):

Pleistocene and other materials for palaeoclimatic research.

Lion Point, Clacton, Essex: implements, Palaeolithic, Neo, BA, and soil samples.

Walton-on-the-Naze, Essex: implements and soil samples from Pleistocene, Neo, and BA sites.

Northern France: Palaeolithic implements and samples.

Italy: Palaeolithic implements and samples.

Annual Report 2 (1938), 12–13:

Palestine: Chalcolithic and EBA material from various sites given by Palestine Museum, Jerusalem.

Jericho: Chalco-EBA sherds from Professor Garstang.

Tell Duweir (Lachish): EIA pottery from Wellcome-Marston Expedition.

South Palestine: Nabataean and sigillata sherds, from Colt Archaeological Expedition.

Arab material (mainly), from Mr Oliver Myers.

Atchana, Syria: pottery from Sir Leonard Woolley's excavations.

Sakje Geuze, Cilicia, Turkey: EBA-Hittite pottery, from Professor Garstang's excavations.

Cyprus: Early Cypriot to Hellenistic pottery from Mr E. S. M. Perowne.

Crete: sherds from Miss Olga Tufnell.

Leicester: stratified Roman sherds.

Wroxeter: stratified Roman sherds.

Uganda Protectorate: stone implements from Mr G. P. O'Brien, many types (Geochronology, p. 25).

Annual Report 3 (1946), 7, 9:

Cyprus: EBA pottery from Vounous, presented by Sir Charles Marston.

Atchana, Syria: more pottery from Sir Leonard Woolley's excavations.

Brak and neighbouring sites, Syria: material from Mr M. E. L. Mallowan's excavations.

Scandinavian Neo stone implements from Humbla Collection, from Dr W. L. Hildburgh (see also *AR* 4 (1947), 9).

Annual Report 4 (1947):

Palestine: Sir Charles Marston Collection.

Upper Palaeolithic West European stone tools series, classified by Abbé Bouysonnie.

Annual Report 5 (1948), 11:

- Spanish Morocco, Canary Islands and Spain: sherds from Seminario de la Historia primitiva del Hombre, Universidad de Madrid.
- Palaeoliths lent by Reading Museum.
- Palestine Exploration Fund: semi-permanent loan of 'very large assortment of pottery and other objects'.
- Mersin, Cilicia: type series of sherds from Professor Garstang's excavations.
- Harappa, India: pottery, terracottas and seals.
- Arikamedhu, India: pottery. These last two entries both via the Director-General of Archaeology [Wheeler].
- Environmental (p. 15; see also *AR* 6, p. 3):
- Lower Palaeolithic implements from Wellcome Historical Medical Museum (Dr Ashworth Underwood and Mr A. D. Lacaille).
- African Lower Palaeolithic implements from van Riet Lowe (South Africa), Summers (Southern Rhodesia), Clark (Northern Rhodesia) and Leakey (Kenya).

Annual Report 6 (1949), 3:

- Hassuna, Iraq: sherds.
- Mersin, Cilicia: sherds.
- Jericho, Palestine: small finds, from Lady Marston.
- Gandhara sculptures, three, from Dr W. L. Hildburgh.
- Pakistan: prehistoric sherds, via Miss Beatrice de Cardi.
p. 33:
- Slindon, Sussex: Acheulian implement, subject of a note in this volume by Edward Pyddoke.
- Environmental, p. 5:
- Corner Collection of Lower Palaeolithic implements, mostly British Acheulian and Levalloisian.

Annual Report 7 (1950), 2:

- Kharga Oasis, Egypt: most of the stone implements excavated by Miss G. Caton-Thompson.
- Tell Duweir, Palestine: type-series of objects excavated by Wellcome-Marston Expedition 1932–38, now presented.
- Environmental, p. 4:
- India: stone implements, large collection made by Dr Zeuner.

Annual Report 8 (1951), 2:

- Cotton Collection of Roman coins and medals, presented by Mrs M. A. Cotton.
- Kimmeridge shale industry, material left by Dr Henrietta Davies.
- Khartoum, Sudan: prehistoric sherds and stone implements.
- Malta: pottery, glass and bones from Commander J. N. Drummond.
- India: 15 palaeoliths from collection of Dr Eliot Curwen.

Baluchistan: sherds, from Colonel D. H. Gordon.

Denmark: type collection of Meso and early Neo core and flake axes from National Museum, Copenhagen.

Annual Report 10 (1953), 3:

Meare Lake Village, Somerset: sherds from excavations by Mr St George Gray.

Annual Report 11 (1953–54), 3:

Flint implements (?British), collection of Mr F. N. Haward.

Annual Report 12 (1954–55), 4:

Peterborough district: Neo and BA pottery and other artefacts collected by Mr Wyman Abbott.

Annual Report & Bulletin 13 (1955–56), 4:

Wiltshire BA and EIA pottery, from Devizes Museum.

Belgium: Neo pottery, from Musées Royaux d'Art et d'Histoire, Brussels.

Bulletin 1 (1958), 78:

Jericho: material from 1956 excavations.

Bulletin 2 (1959), 75:

Nimrud: material from Professor Mallowan's excavations.

Crete: Neo and Minoan sherds, from Professor Evans.

Bulletin 3 (1962), Director's Report for 1958–59, 81:

S. England (mainly): stone implements, Palaeo to BA, from Mr R. H. M. Clayton.

Lipari Islands: LBA sherds.

Crete: Neo sherds. These two items from Professor Evans.

Stone implements from local sites, from Ipswich Museum.

Nahal Oren, Israel: Meso and Neo flint implements, from M. Stekelis.

Director's Report for 1959–60, 94:

Kulli and Shahi-tump, India: sherds, from Director-General of Archaeology.

Arab pottery, modern, from Miss Olga Tufnell.

Czechoslovakia: casts illustrating its prehistory, from J. Böhm.

Vinča, Yugoslavia: selection of material on permanent loan from Birmingham City Museum.

Northern Persia: collection of bronzes and pottery, from Mrs I. Ainley.

Beycesultan, Turkey: potsherds, from British Institute at Ankara.

Shukbah, Palestine: stone implements, from Dr John Waechter.

Bulletin 4 (1964), report for 1960–61:

Thessaly, Greece: Neo sherds, from Dr D. Theocharis.

Little Woodbury, Peacocks Farm and Irish sand-hill sites: sherds and flints, from Mr S. S. Frere.

Switzerland: bronzes, sherds, and an antler sleeve from various sites, from Mr V. Megaw.

Report for 1961–62:

Yarim Tepe, Iran: IA and earlier material, from Mr D. J. Stronach.

Yanik Tepe, Azerbaijan: EBA sherds, from Mr C. Burney.

Tell Amiyak, Turkey: 'Ubaid pottery, from Mr D. J. Stronach.

Bulletin 5–6 (1962–65): report for 1963–64, 123:

Eastern European prehistoric sherds, from Dr Sulimirski.

Bulletin 7 (1967), 150:

Hurst Fen, Eriswell, Suffolk: Neo flints, from Dr H. A. Fawcett, to be added to pottery
from same site, already presented.

No further entries were made.

History of the Collections

The description of 'The Study-Collections' in the first *Annual Report* (1937: 23–8) begins by describing the cases and cabinets to hold the artefacts; curatorial responsibilities were taken seriously from the beginning. But at the start of the war (*AR* 3 (1946), 8–9, 14–15) the entire contents of St John's Lodge were packed up and stored in the basement, in one week, by staff and students. From 1940 the library was re-opened and the building began to function once more. The collections, in the basement and added to periodically with new acquisitions, had a not uneventful time. Despite bombs falling all around St John's Lodge, 'the worst inconvenience caused was the blocking of the main sewer by a bomb in the Inner Circle, which caused the flooding of parts of the basement, and which took many months to repair'. The boxes affected were rescued by the caretaker, Manson, who risked life and limb constantly while in charge of the building. 'The greater part of the collections survived the moves and the incidents of war very well', although most of the building's windows were smashed, the ceilings had come down and vibrations from bombing had caused breakages. In October 1945 a volunteer force spent a weekend bringing the collections and cases upstairs, and cataloguing began anew.

The Secretary, Kathleen Kenyon, was also Honorary Curator, but could only find spare moments to concentrate on the Palestinian collection until freed from the secretaryship in the 1947–48 session. Each department, it was evidently hoped, would look after its own speciality and Edward Pyddoke was appointed part-time assistant by the Department of European Archaeology in the session 1946–47 with responsibility for 'arranging and cataloguing the relevant section of the collection' (*AR* 4 (1947), 8). It was hoped that a new post in the Archaeology of the Roman Provinces would provide someone with the skill to deal with the Roman pottery. Miss Marjorie Maitland Howard worked with Mr Pyddoke on the stone implements and other parts of the Department of Environmental Archaeology's collections (*AR* 5 (1948), 14; *AR* 6 (1949), 4). In 1950, however (*AR* 7, 3) it had to be reported that 'the growth of the collections had made it necessary to appoint a temporary Register Clerk to record current accessions and bring the registration of specimens up to date'. (It was reported on the same page that the Librarian's office had been evacuated, having been condemned by the Borough Surveyor, and that the building was at last about to be redecorated, for the first

time since 1939.) The new 'temporary' Register Clerk was Judy Philips, who had been a Diploma student. She had much work to do, since the removal of the Institute to new premises was already envisaged (in *AR* 11 (1953–54), 3) and 'indexes of sites and types' were still 'urgently needed'. In the 1956–57 Report (*Bulletin* 1 (1958), 78) the Librarian, Miss Joan du Plat Taylor, is also described as curator of collections and officially Judy Philips was included under library staff until her retirement. She is first listed as Collections Clerk in the library report for the 1959–60 session (*Bulletin* 3, 1962). During the 1956–57 session the Palestinian Collection was packed up ready for transfer to the new building in Gordon Square and mention is made of Miss du Plat Taylor's work in organising the collections to make best use of the new standard storage fittings.

Following the Institute's move to Gordon Square, only the occasional new acquisition is mentioned in the Annual Reports (published once more separately from the Bulletin after *Bulletin* 12). After the report for the 1965–66 session the collections are not mentioned again until *Annual Report* 34 (1976–77), 1, when Judy Philips retired. 'Her departure severs one of the last remaining links with the Institute's Regent's Park days . . . Her post will not be filled because of the financial situation.' Each department was now made responsible for the documentation and care of its own collection. The Prehistory Department collections, now including the worked stone from the Environmental Department, and the largest after the Palestinian, were sorted and re-catalogued, and storage locations added, under a one-year contract by Jill Craddock in 1978–79. The other departmental holdings have not been given similar treatment, but the Palestinian Collections have always been comparatively well known and in use for teaching and research, and more visible in their upstairs rooms than in the basement storage racks; while the Roman Department collections are now also all upstairs and in use. This latter group was recently examined and weeded out by the department's staff and the records made available in the department secretary's office. The Human Environment Department (once the Department of Environmental Archaeology) now has a technician, Jenny Brigham, who is in charge of curating the department's collections, now used exclusively for research; there are many boxes of unlabelled material and few records.

Disposal of objects

There are a few notices in the published records of material leaving the collections: unspecified objects were exchanged in the 1955–56 session for pottery from the Devizes Museum and the Musées Royaux d'Art et d'Histoire in Brussels, and 'duplicates' were given to Mr V. Megaw in the 1960–61 session for the Department of Archaeology of Sydney University, in exchange for material from Swiss sites. The objects sent to Sydney are crossed out in the old loose-leaf 'European Department' accessions registers and marked 'Sydney': these are listed in Table 1. Some Indian material has been returned to SOAS. Recently four Harappan seals and some pottery were sent to the British Museum; while the Wellcome-Marston Expedition's Lachish (Tell Duweir) collection, given to the Institute in 1950, has been sold to the British Museum. Due to the group's size and importance a museum was considered a more suitable home for it, despite the qualms felt over the selling of a munificent gift. The Roman Department has returned a certain amount of its material to museums.

Table 1: 'To Sydney' entries in Accessions Registers (European Department registers: Britain and Europe Z48/55 - 64/428)

Z48/71	Campello, Alicanti	Painted rim sherd
Z50/15	Vilanova de S Pedro	2 microform hollow based arrow heads
Z50/16	Vilanova de S Pedro	2 hollow based arrow heads
Z50/6	Vilanova de S Pedro	1 flint blade
Z50/54	Vilanova de S Pedro	1 plain clay plaque
51/104	Bergen Nadar, Malta	Red incised potsherd
51/145	Kordin, Malta	Grey-black burnished sherd, incised rim
51/3388	Kordin, Malta	Rim sherd, decor after firing
51/3392	Kordin, Malta	Rim sherd, decor after firing
51/3507	Borg-en-Nadur, Malta	Rim sherd, 'haematite slip' o/s, red on buff painted i/s
51/3524	Borg-en-Nadur, Malta	Sherd, 'haematite slip'
51/3536	Borg-en-Nadur, Malta	Sherd, 'haematite slip' rim
53/385	Eilean-an-Tighe	3 rimsherds. Stage I. Internal bevel plain
54/40	Toome, Co. Antrim	1 Bann point with worked tang (flint). Neo.
54/64	Cranwich area, Norfolk	2 Neo or BA scrapers
54/126	Mundford Breck, Cranwich area, Norfolk	Axe, Neo or BA
54/167	Middle Breck, Norfolk	Plano-convex knife, BA (marked 197A)
55/689	Tarxien, Malta	Sherd, coarse ware, applied scale pattern
55/691	Tarxien, Malta	Sherd, coarse ware, scored, with lug or cordon
55/1761	Tarxien, Malta	Sherd, incised decor, ?white inlay
56/314	Woodhenge	1 sherd grooved ware (Rinyo-Clacton)
56/465	Vaux-et-Borset, Liege, Fond 8	Rim sherd, incised and impressed decor. Omalian Neo.
56/814	Scandinavia	Ground axe, miniature
56/819	Scandinavia	Polished adze
56/857	Scandinavia	Ground and pecked adze
56/889	Scandinavia	Flaked and polished axe
56/893	Scandinavia	Flaked and polished axe
56/951	Scandinavia	Polished chisel
56/954	Scandinavia	Polished gouge
56/967	Scandinavia	Dagger, fishtailed
56/997	Scandinavia	Sickle
56/1029	Scandinavia	Arrowhead
56/1051	Scandinavia	Boat axe
56/1057	(Nagotsonden) Bohustan, Scandinavia D/760	Boat axe end only
56/1059	Antaglagon fronskone, Scandinavia D/567	Battle axe
56/1128	Halland, Scandinavia D/188	'Whetstone' or netsinker, oval, grooved
56/1139	Halland, Scandinavia D/109	Spindlewhorl, Iron Age
56/1143	Halland, Scandinavia D/296	Spindlewhorl, Iron Age
56/1147	Halland, Scandinavia D/473	Waisted mace head
56/1608	Fayum D/1057	Celt
56/1651	Fayum D/1104	Bifacial knife, serrated edge

Table 1—cont

56/1655	Fayum D/1109	Bifacial knife
56/1662	Fayum D/1116	Bifacial knife, serrated edge
56/1754	Fayum D/1217	Bifacial knife
56/1763	Fayum D/1228	Hollowbased arrowhead
56/1771	Fayum	Shouldered point, broken
56/2294	Los Millares, Spain	Rim sherd fine red ware
56/2298	Los Millares, Spain	Rim sherd
56/2312	Tabernas, Spain	Fragment of pottery plaque with 2 perforations
56/2319	Tabernas, Spain	Rim sherd with lug
56/2386	Cabeza de la Bastida, Spain	1 fragment slag
56/2388	Cabezo Redondo, Spain	Rim sherd
59/181	Maiden Castle MCQ. P59 Pit Q8, 1 & 2	Rim sherd, pierced horizontal lug, coarse ware red-brown ware. Neolithic
59/192	Maiden Castle MCQ. P35. 2. 170	Sherd rim and plain horizontal lug. Neo.

Table 2 Prehistory Department Collections. Summary by country of non-British material

Within the following headings, the card index is arranged alphabetically by site, not period divisions.

AFRICA: Largely stone implements. Sites very scattered.

AUSTRALIA: Entirely stone implements.

AUSTRIA: Only 4 cards; pottery, slag, stone implements.

BALEARIC ISLANDS: Mostly sherds. Some locations vague; some Roman material.

BELGIUM: Palaeo-Neo; sherds, some stone.

BULGARIA: 2 cards only, sherds.

CANARY ISLANDS: Sherds, various sites.

CZECHOSLOVAKIA: Sherds, stone implements.

EGYPT: Stone implements, various; and Kharga Oasis, various objects.

FRANCE: A lot—stone implements, and pottery various down to Iron Age.

GERMANY: Stone, pots; including Roman-medieval (Pfünz). Not much in all.

GIBRALTAR: Gorhams Cave; graphite crayon and sherds.

GREECE: Nearly all pottery, Neo to Hellenistic.

HUNGARY: 2 cards, pots and one stone axe.

INDIA: Mostly stone implements.

IRAN/IRAQ: 3 cards stone implements, 1 pottery.

IRELAND: Not a great deal; almost entirely stone implements.

ISRAEL: One card, stone implements.

ITALY AND ISLANDS i.e. Aeolian Islands, Sicily: Mostly pottery; some stone implements, some bronzes.

JAMAICA: Several cards; pottery, shell, coral.

JAPAN: Obsidian, 1 card.

JORDAN: All stone implements, and Zeuner excavation material.

LEBANON: As Jordan.

MALTA: Mostly sherds; some bone etc.

NEW ZEALAND: 1 card, stone, ?forgery.

NORTH AMERICA: All stone implements.

PACIFIC ISLANDS: 2 cards, vague sites, stone implements.

PAKISTAN: Quite a lot, and varies; many sherds.

PALESTINE: Mostly stone implements.

POLAND: Only 5 cards, sherds and some stone implements.

Table 2—*cont*

- PORUGAL: 3 cards, sherds and miscellaneous.
- ROMANIA: (early) pottery.
- SAUDI ARABIA: 1 card, stone implements.
- SCANDINAVIA: A lot, almost entirely stone implements. The first 2 cards give provenances.
- SOUTH AMERICA: Mostly pottery.
- SPAIN: Sherds, some stone, bronzes; some Roman.
- SWITZERLAND: Varied, 4 cards, lake dwellings.
- THAILAND: Pottery, mostly; 4 cards.
- TURKEY: 3 sites, mostly sherds.
- USSR: 5 cards, sherds.
- YUGOSLAVIA: 3 cards, pottery and miscellaneous.
- MISCELLANEOUS: Site names, but location of material unknown; 8 cards, plus 2 cards of material without provenance.

Table 3: Neolithic material from British sites

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- No indication of quantity is given here; the date is the year of accession.
- Pike of Stickle, Langdale: worked stone. 1948.
- Eilean-an-Tighe: sherds. 1953, 1959.
- Weeting, Suffolk: worked stone. 1954.
- Mundford Breck, Norfolk: worked stone. 1954.
- Cranwich area, Norfolk: worked stone. 1954.
- Middle Breck, Norfolk: worked stone. 1954.
- Thetford, Norfolk: worked stone. 1954.
- Windmill Hill, Wilts.: worked stone. 1954.
- St Margaret's, Kent: worked stone. 1954.
- Salvington, Sussex: worked stone. 1959.
- Craig Llwyd: worked stone. 1959.
- Grimes Graves, Norfolk: worked stone. 1959, 1968.
- Whitehawk Camp, Brighton, Sussex: sherds and worked stone. 1959.
- Maiden Castle, Dorset: sherds, worked stone, bone points. 1959, 1968.
- Kimmeridge, Dorset: bone point. 1959.
- Cissbury, Sussex: worked stone. 1960.
- Mildenhall, Suffolk: worked stone. 1960.
- Weaverthorpe, Yorks: worked stone. 1962.
- Hurst Fen, Eriswell, Suffolk: sherds, worked stone. 1966.
- Skara Brae, Orkney: sherds, bone, worked stone. 1968.
- Rinyo, Rousay, Orkney: sherds, worked stone. 1968.
- Castle Hill, Fairlight Glen, Hastings: Neo/BA arrowheads. 1954.
- Icklingham, Suffolk: Neo/BA arrowheads.
- Woodhenge, Wilts.: Beaker and grooved ware sherds. 1956.
- Peacock's Farm, Shippea Hill, Cambs.: Late Neo and Beaker sherds: 1960.
- Lower Halstow, Kent: Neo/BA worked stone. 1952.
- Seamer Moor, Yorks.: Neo/BA worked stone. 1954.
- Heslerton Wold, Yorks.: Neo/BA worked stone. 1954.
- Rockley, Wilts.: Collared urn rim. 1956.
- Temple Bottom, Wilts.: Collared urn rim. 1956.
- Avebury Barrow, Wilts.: Collared urn rim. 1956.
- Lakenheath, Suffolk: BA arrowhead. 1956.
- Bekesbourne, East Kent: BA sherd. 1962.
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Access and existing records

Anyone wishing to examine Institute material should write to the appropriate Head of Department. This is the position in the summer of 1986, before the merger with University College, and there is no direct connection with the UC Egyptology or Medieval Archaeology departments. Each Institute department (Prehistory, including Far East and Latin America, Roman Provinces, Near East, Human Environment) has its own records, kept by the department secretary. These records are primarily by site and period. In the Registrar's office are the old 'European Department' accessions registers, which cover the bulk of the collections apart from Palestinian from 1948 to 1964, but these are in order of the date of accession only.

In conclusion, this brief outline has been necessarily discursive and the only recompense I can offer is an illustration of the range of the collections in a summary of non-British material in the Department of Prehistory (Table 2). To produce a similar round-up of the British material is impossible in this context but Table 3 (incomplete and showing only Neolithic sites) may whet the appetites of researchers.

Fieldwork of the Institute of Archaeology

Great Britain

1. Ms Miranda Armour-Chelu	Research student	Zooarchaeological study of mammalian remains from selected British sites
2. Tony Barham	Staff	Magnetic susceptibility examination of archaeological sediments, e.g. Roman hearths at Leadenhall, London
3. Ms Marie-Louise Bowen	Student	Comparison of two regional archaeology units in Oxford and Clwyd
4. Don Brothwell	Staff	Study of Iron Age bones from Danebury
5. Don Brothwell and Tim Holden	Staff	Study of the stomach contents and faeces of Lindow man
6. Jeremy Butler	Research student	The landscape archaeology of Dartmoor
7. Stephen Carter	Research student	Taphonomy of land-snail assemblages in chalkland areas of southern England
8. Ms Caroline Cartwright	FAU (Field Archaeology Unit) staff	Study of charcoal and wood from Stonea Grange, Cambs, Mount Grace Priory, Yorks, and Sussex sites
9. Ms Carolyne Chiasson	Research student	Study of monumental bathhouses in the Roman empire and excavation at Strageath
10. Ms Patricia Christie	Staff	Study of Cornish barrows
11. Alan Clapham	Research student	Study of Dark Age agriculture at Castle Copse, Wiltshire, also plant remains from Oakbank Crannog, Scotland
12. Dr Rod Clough	Staff	The metallurgy of Romano-British iron-work of the Weald
13. Keith Dobney	Research assistant	Dental calculus studies from Early Medieval and Anglo-Saxon York
14. Dr Peter Drewett	Staff	Preseli Mountains survey, Dyfed, and excavations of Lewes Castle, Sussex
15. Ms Maria Fabrizi	Research student	Study of the corrosion of bronzes, concentrating on British material
16. Ms Jane Faiers	Research student	Study of Roman pottery from Wroxeter
17. Nicholas Friend	Research student	Study of villa forms in south-west Britain
18. Nicholas Fuentes	Research student	Study of Roman archaeology in London

19. Mark Gardiner	FAU staff	Anglo-Saxon settlement in the Adur Valley, Sussex; Medieval rural settlement and society in the East Sussex Weald; Medieval settlement and land reclamation in Romney Marsh
20. Brian Gilmour	Research student	Metallurgy of Anglo-Saxon iron blades
21. Dr Dafydd Griffiths	Staff	Provenance studies of flaked chert tools of western Scotland by ESR spectroscopy
22. Ms Valerie Griggs	Student	Personal names in Roman Britain
23. Michael Hammerson	Research student	Study of 4th century Roman coins in south-east England
24. James Hart	Student	Neolithic environments on the Chalkland
25. Mark Hassall	Staff	Roman inscriptions in Britain
26. Ms Sara Haynes	Student	Statistical analysis of prehistoric assemblages of land snails
27. Jonathan Henderson	Student	Studies of population in Roman Britain
28. Gordon Hillman	Staff	Studies of crop-husbandry and plant food exploitation at a number of British sites and Lindow man's stomach content analysis
29. Robin Holgate	FAU staff	Prehistoric settlement of the weald; Neolithic-Bronze Age industrial activity on the South Downs and Thames Valley
30. Tim Holden	Research student	Crop husbandry and crop processing at medieval Rhuddlan, Clwyd
31. Ms Debbie Jackson	Research student	Study of bird bones from British sites
32. Robert Kenyon	Research student	Study of native copies of coins of Claudius in Britain
33. Ms Shila King	Research student	Study of the Iron Age in Essex
34. Peter Leach and Dr Peter Drewett	Staff	Medieval excavations and landscape survey at Bore Place, Kent
35. Dr Richard Macphail	Staff	Study of the soil micromorphology of British sites
36. Ms Margaret Maher Ms Myfanwy Stewart	Research students	Excavation of a Mesolithic site on Hampstead Heath, London
37. Colin Martin	Student	Roman settlement in the Darent Valley
38. John McNabb	Research student	Study of the Lower Palaeolithic of the Thames Valley
39. Ms Jane Murray	Student	Magnetic susceptibility and archaeological sediments
40. Dr Mark Newcomer	Staff	Stone tool technology at Pontnewydd cave, North Wales
41. Giles Pattison	Student	Desertion, shrinkage and survival of villages of Flitcham-cum-Appleton



42. Dr Richard Reece	Staff	Studies of coins from Lincoln, Colchester, Gloucester, <i>et al.</i>
43. Mark Roberts	FAU staff	Excavation of a Lower Palaeolithic site at Boxgrove, West Sussex
44. David Rudling	FAU staff	Iron Age settlements and enclosures in Sussex and Romano-British settlements
45. David Rudling Mark Gardiner	FAU staff	Origin of Sussex towns
46. Timothy Sapwell	Student	A report on the Romano-British pottery kilns at Morley St Peter, Norfolk
47. Ms Delwen Samuel	Research student	Roman crops from Glamorgan, Wales
48. Ms Katherine Sawyer	Research student	Study of Bronze Age burials in Wessex
49. Dr Rob Scaife	Staff	Palaeobotanical studies of sites in southern England; Flood plain alluviation in south-east England; The monk's impact on the vegetation of Iona from the 7th century onwards
50. Ms Kathryn Schluttenhofer	Student	Variation in enamel hypoplasia in past peoples
51. John Shepherd	Research student	Study of Roman glass in Britain
52. Ms Rachel Swan	Research student	Early metallurgy of British silver
53. Dr Ken Thomas	Staff	Environment and settlement on the Chalklands
54. Ms Zoe Tomlinson	Student	Application of computer graphics to British prehistory
55. Steve Trow	Research student	Late Iron Age to Roman times in Gloucester
56. David Underwood	Student	Flint assemblages from Neolithic and Bronze Age burials in the Thames Valley
57. Franco Vartuca	Student	Archaeological illustration: an assessment from the past to the present
58. Ms Simone Warr	Student	The methodology in assessing Harris lines in tibias
59. Ms Jacqui Watson	Research student	Freeze drying of waterlogged British wood
60. Mark Wiggins	Student	The problem of late Roman London
61. Professor John Wilkes	Staff	Excavation of the Roman fort at Strageath Crieff, Perthshire, Scotland
62. Ms Susan Wright	Student	Dragonesque brooches; their origins, development and distribution
Europe		
63. Ms Eugenia Adam	Research student	Excavation of an Upper Palaeolithic rock shelter in Epirus, Greece
64. Dr Paul Arthur	Research student	Excavation and fieldwork in the Naples area, Campania, including Pompeii

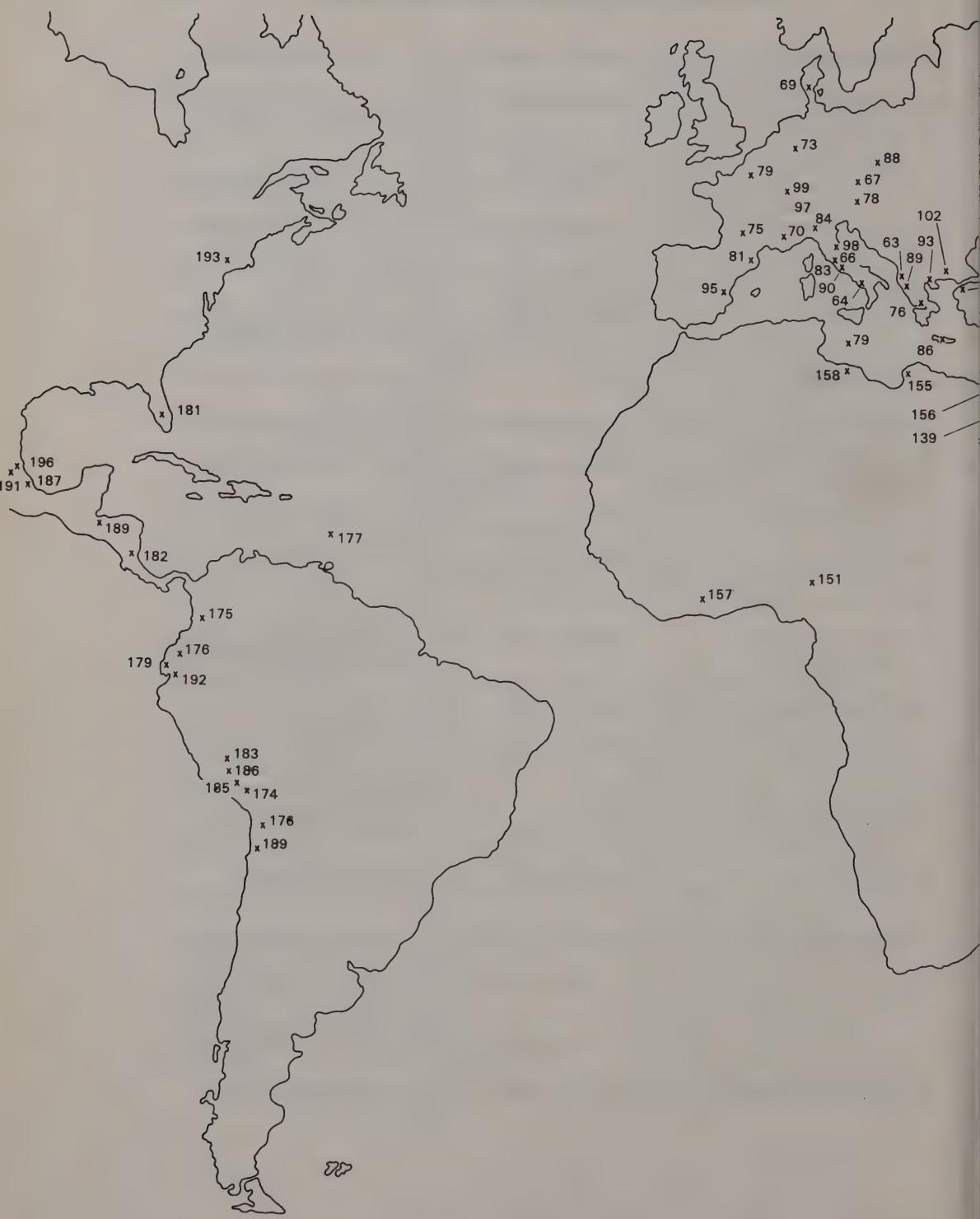
65. Ms Olivia Belle	Student	Archaeology of women: inscriptions and burials
66. James Betteridge	Research student	Study of the archaeology of early Rome
67. Jim Black	Staff	Conservation of wall-paintings at the Bundesdenkmalamt, Vienna
68. Ms Francesca Brewer	Research student	Study of the metallurgy of classical bronzes from Italy
69. Don Brothwell and Tim Holden	Staff	Study of the stomach contents and faeces of Grauballe man, Denmark
70. Ms Fiona Cameron	Research student	Study of Roman sites in the Var region, France
71. Ms Victoria Cassely	Student	Wives and families of Roman army officers
72. Peter Connolly	Research Associate	Roman military equipment
73. Damian de Rosa	Student	Roman cavalry harness from Xanten, Germany
74. Professor John Evans	Staff	Prehistoric archaeology of islands in the Mediterranean
75. Ms Beatrice Illett Fleury	Research student	Study of Late Iron Age pottery from the Soissons region of France
76. Gordon Hillman	Staff	Study of the grain crops from the burnt 'granary' at Mycenae, 1100 BC, Greece
77. Andrew Hobley	Research student	Study of circulation of Roman coins in Europe, AD 81–192
78. Professor Roy Hodson	Staff	Study of material from Hallstatt, Austria
79. Robin Holgate	FAU staff	Prehistoric settlement of the Plain of Caen, Normandy, northern France
80. Peter Horne	Research student	Study of Romano-Celtic temples
81. Dr Simon Keay	Research student	Excavations and fieldwork in the area of Tarragona, Spain
82. Nikos Kokkinos	Student	Material for the study of Antonia Augusta
83. Ms Tamara Lewit	Research student	Study of the economy and archaeology of Rome, 3rd–4th century AD
84. Dr Richard Macphail	Staff	Study of the soil micromorphology of sites in the Val Chisone and the Val Susa in the Italian Alps
85. Professor John Mann	Research Associate	Roman legionary stations
86. Ms Katia Mantei	Research student	Study of the Neolithic–Early Bronze Age transition in Crete
87. David Marchant	Student	The Castra Praetoria and its garrison
88. Dr John Nandris	Staff	Highland zone ethno-archaeology project in the Carpathian, Pindos and Dalmatian mountains and in the Tatra Mountains of southern Poland
89. Dr Mark Newcomer	Staff	Excavation of a Palaeolithic open site at Pincevent, France, and of a palaeolithic cave site in Epirus, Greece

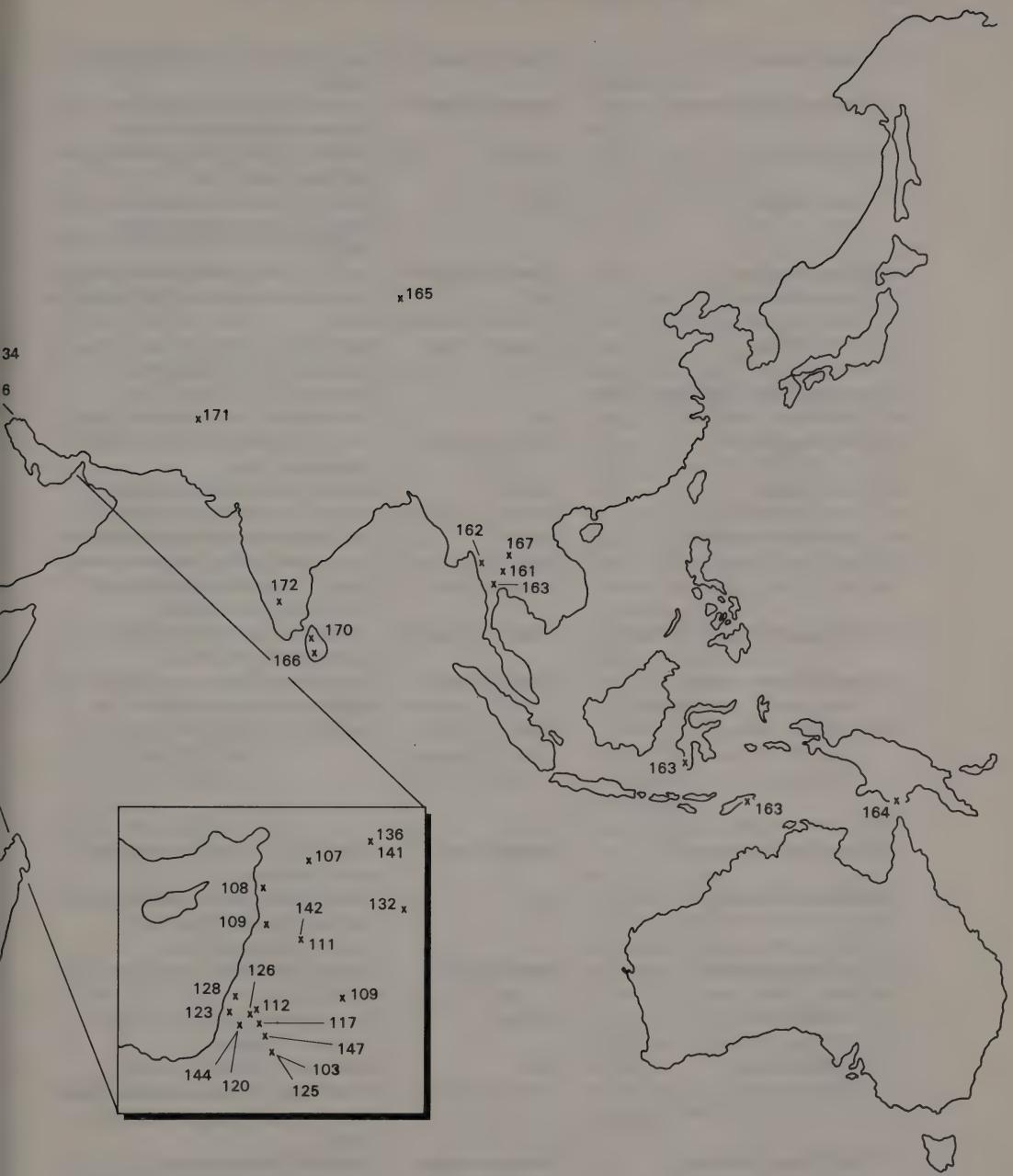
90. Albertus Nijboer	Student	Provenance study on ceramics from Saltricum (Latium), Italy
91. Ms Marcia Okun	Research student	Study of the Roman frontiers on the Rhine-Danube
92. Brian Oldham	Research student	Study of Iron Age swords in central Europe
93. Ms Effie Photos	Research student	Study of early extractive iron metallurgy in northern Greece
94. Mark Redknapp	Research student	Study of Late Roman pottery in the Eifel and excavation of a Roman villa
95. Paul Reynolds	Research student	Study of imported Roman fine wares of south-east Spain and the excavation of a Roman villa near Valencia
96. Dr Margaret Roxan	Research Associate	Roman military diplomas
97. Dr Rob Scaife	Staff	Study of the pollen remains from sites in the Val Chisone and the Val Susa in Italy; also work in Brescia, Italy
98. Robin Skeates	Student	Sedimentation in the settlement of the Fucino Basin of Central Italy
99. Ms Scarlett Walker	Student	Roman posting stations
100. Kit Watson	Student	Pudding Pan Rock wreck and other Samian wares
101. Jason Wood	Research student	Study of the 4th century defences in France
102. Ms Paraskeui Yiouni	Research student	Study of sixth millennium pottery of Neolithic Macedonia

The Levant and the Middle East

103. Dr Khairieh Amr	Research student	A neutron activation study of Nabatean pottery from Petra, Jordan
104. Marcus Arguelles	Research student	Study of Byzantine coinage from the site of Sarafand, Lebanon
105. Ms Kathryn Ataman	Research student	Study of stone tools from Can Hasan III, Anatolia
106. Amr al-Azm	Research student	The ethno-archaeology of present-day villages in north and west Syria
108. Dr Chris Bergman	Research Associate	Study of flint tools from Ksar Akil, Lebanon
109. Dr Alison Betts	Research student	Excavation of Neolithic sites in the Black Basalt Desert, Jordan
110. Roger Bland	Research student	Study of 3rd century AD Roman coins in the eastern Mediterranean
111. Stephen Bourke	Research student	Study of pottery of the Middle-Late Bronze Age transition from Tell Nebi Mend, Syria, and Pella, Jordan
112. Julian Bowsher	Research student	Excavations at Jerash, Jordan
113. Jim Black	Staff	Survey of conservation requirements for the national museum, Sana'a, North Yemen

114. Kevin Butcher	Research student	Study of Greek Imperial coins in the Levant
115. Ms Ann Butler and Mark Nesbit	Research student	Collection of reference material for the study of food legumes in prehistory Turkey
116. Michael Charles	Research student	Study of crop husbandry in Mesopotamia during late fourth and early third millennium, Iraq
117. Ms Susan College	Research Associate	Epipalaeolithic exploitation of plant resources at Wadi Hammeh in the Jordan Valley, Jordan
118. Ms Vesta Curtis	Research student	Study of Parthian costume
119. Keith Dobney	Research assistant	Animal and human bone assemblages from Mohammed Arab, north-west Iraq
120. Peter Dorrell	Staff	British Museum excavations at Tell es-Saidiyeh, Jordan
121. Donald Easton	Research student	Study of the chronology of Troy
122. Ms Mercy Feldbrugge	Student	The Greek cults in the Levant
123. Robert Fellner	Research student	Study of the Upper Palaeolithic in Israel
124. Ms Katelin Flavin	Student	Study of the reliefs in Sennacherib's palace at Nineveh
125. Ms Eve French	Research assistant	Study of Nabatean figurines of Petra, Jordan
126. Dr Anthony Frendo	Research student	Study of Late Bronze Age to Iron Age pottery from Deir Allar, Jordan
127. Abdul Aziz al-Ghazzi and Hamid Mazroo	Research students	Study of material from the University of Riyadh excavations at Al-Fau, Saudi Arabia
128. Simon Gibson	Research student	Study of ancient field systems and settlement in Israel
129. Roger Grace	Research student	Study and microwear analysis of stone tools from Anatolia
130. Professor David Harris Gordon Hillman Ms Sue College	Staff Staff Research student	Survey of modern plant communities as an aid to interpretation of the archaeobotanic evidence in Turkey and Syria
131. Jonathan Hather	Research student	Criteria for the identification of vegetative tissues of tuberous plants, Jordan
132. Gordan Hillman	Staff	Study of early agriculture of a number of sites in Jordan, Syria and Turkey; in particular Tell Abu Hureyra, north Syria; Can Hasan III and Catal Huyuk, Turkey; study of pre-agrarian (forager) diet and subsistence in Near Eastern sites
133. Ms Linda Carless Hulin	Research student	Study of eastern Mediterranean fenestrated cult stands





134. Simon James	Research student	Study of material excavated at Dura Europos
135. Ms Berrin Kusatman	Research student	Zooarchaeological study of Near Eastern pig domestication sites
136. Tim Matney	Student	Technical study of the eye-idols from Tell Brak, north Syria
137. James Mellaart	Staff	Study of neolithic wall-paintings from the site of Catal Huyuk, Konya, Turkey
138. Fouad Momena	Research student	Study of South Arabian architecture
139. Dr John Nandris	Staff	An ethno-historical study of bedouin in Sinai
140. Steven Nicklas	Research student	Study of 4th century Roman coinage in the eastern Roman Empire
141. Professor Emeritus David Oates		Excavation at Tell Brak, Syria
142. Peter Parr	Staff	Excavation at Tell Nebi Mend, Syria
143. Ms Irene Levi Sala	Research student	Study of techniques of flint working from sites in Israel
144. David Stacey	Student	Jericho, the southern cemetery
145. Ms Maria Trentin	Research student	Study of the Uruk period in Syria
146. Christian Tutundjian	Student	Egyptian motifs on Phoenician ivories
147. Ms Kathy Tubb	Staff	The excavation and conservation of a group of lime plaster figurines 8000 BP from Ain Ghazal, Jordan
148. Dr Romana Unger Hamilton	Research Associate	Tools and plants of early plant husbandry in the Levant
149. Ms Fanchette Vittoz	Research student	Study of Late Roman and Early Byzantine burials in Galilee
150. Ms Karin Weiss	Research student	Study of the rock art of Anatolia

Africa

151. Raymond Asombang	Research student	Late Stone Age cave sites in north-west Cameroon
152. Gordon Hillman	Staff	Riverine subsistence and diet during the Late Palaeolithic at Wadi Kubbaniya, Upper Egypt
153. Ms Diane Holmes	Research student	Study of Predynastic stone tools from Upper Egypt
154. Nicholas Luke	Student	Comparison of rock art of Southern Africa and Tanzania
155. Ms Elizabeth Pye	Staff	Iconography, technology and conservation of Roman wallpaintings from Berenice (ancient Benghazi), Libya
156. Ms Ursula Thanheizer	Research student	Impact of Hykos influence on agriculture in the Nile Delta in the second millennium BC.
157. Dr Ann Stahl	Staff	Banda village research project, Ghana

158. Hafez Walda	Research student	Analysis of sculptures from Lepcis Magna, Libya
159. John Waton	Research student	The metrology of Egyptian stone weights
160. Ms Margot Wright	Staff	The extraction of papyrus from Egyptian cartonnage

South and East Asia and Australasia

161. Ms Anna Bennett	Research student	Excavations at a mining and smelting site at Khao Phuka, Thailand
162. Ms Vanessa Coote	Research student	Early base metal mining in West Thailand
163. Dr Ian Glover	Staff	Ban Don Ta excavations, Thailand; cave excavations at Ulu Leang and Leang Burung, south Sulawesi, Indonesia; cave excavations at Lie Siri, Uair Bobo and Bui Ceri Uato, East Timor
164. Professor David Harris Tony Barham Ms Barbara Ghaleb Ms Sharon Budworth	Staff Staff Research student Research student	Study of archaeology and palaeoenvironment of the Torres Strait, involving excavation, survey, ethnoarchaeology sedimentology and palynology
165. Ms Mira Hyams	Research student	Early historical crop husbandry in Xinjiang, western China
166. Ms Gillian Juleff	Research student	Study of early metallurgy of Sri Lanka
167. Ms Elizabeth Moore	Research student	Early moated sites in north-east Thailand
168. Ms Isobel Pritchard	Research student	Ancient Indian seal technology
169. Ms Sita Ramaseshan	Research student	Deterioration and conservation of Indian paper
170. Dr Nigel Seeley Ms Gillian Juleff	Staff Research student	Supplying conservation services to six sites as part of the Cultural Triangle project, Sri Lanka
171. Dr Ken Thomas	Staff	Excavation of three sites near Bannu, North West Frontier, Pakistan; study in relation to the settlement patterns and the environment
172. Ms Paula Turner	Research student	A study of Roman coins in southern India and their relation to local coinage using metallurgical and other methods
173. Ms Susan Wales	Research student	A study of the chemistry of coprolites including material from Thailand

The Americas

174. Ms Elizabeth Baquedano	Research student	Study of Aztec sculpture
175. Dr Warwick Bray	Staff	Excavations at Calima, Colombia

176. Don Brothwell and Tim Holden	Staff Research student	Study of the stomach contents and faeces of preserved bodies from Aucan and Cusichaca, Peru, from Tiahuanaco, Bolivia, and from Leiva, Colombia
177. Dr Peter Drewett, Ms Lysbeth Drewett and Ms Caroline Cartwright	Staff FAU staff FAU staff	Survey of Prehistoric Barbados, West Indies
178. Ms Elizabeth Currie	Research student	Study of the Jambeli culture of Ecuador
179. Ms Pia Hahn	Research student	Study of coastal Ecuadorian stonework
180. Michael Hoadley	Student	The terminal Pleistocene extinction of the horse in North America
181. Ms Gaynor Jackson	Student	The Glades Indians of South Florida
182. Ms Ursula Jones	Research student	Study of Central American stonework
183. Dr Ann Kendal, Ms Sara Lunt and Ms Gillian Hey	Research Associate	The Cusichaca Valley Project, Peru
184. Gerald Kennedy	Research students Student	Hondurian Babilonia polychrome ceramics
185. Dr Frank Meddens	Research Associate	Excavations in the Chicha Valley, Peru
186. Ms Alexandra Morgan	Research student	Study of Peruvian figurines
187. Robert Novella	Student	Shell artefacts from Western Mexico
188. Ms Linda Salati	Research student	Study of early Maya ceramics
189. Dr Rob Scaife	Staff	Study of pollen and phytoliths of the pre-Inca settlement site of Calama, northern Chile
190. Dr David Scott	Staff	Study of the ancient metallurgy of gold, silver and copper alloys from Colombia and Ecuador
191. Ms Sue Scott	Research student	Study of terracotta figurines from Teotihuacan, Mexico
192. Dr Peter Stahl	Research Associate	Study of ancient fauna of the Valdavia Culture, Ecuador
193. Dr Ken Thomas	Staff	Iroquoian settlement ecology in Upper New York State
194. Ms Mercedes Velasco	Student	Mesoamerica/Central America
195. Bruce Welch	Research student	Study of Maya burials
196. Eduardo Williams	Research student	Study of West Mexican stonework

The Institute of Archaeology Jubilee Programme

In April 1987 the Institute of Archaeology was 50 years old. This Jubilee was celebrated in a number of ways. One of the first events was a series of eight public lectures presented between January and March under the title 'Retrospect and Prospect'. The speakers were members of staff and invited guest lecturers from outside institutions. Their papers are printed in this Jubilee issue of the Institute's Bulletin (see Contents). The sequence of the lectures was:

- 20 January Professor J. D. Evans – The first fifty years – and after
27 January Dr P. L. Drewett – Field archaeology and the Institute
3 February D. R. Brothwell – Biophilosophical aspects of archaeology
10 February Dr N. J. Seeley – Archaeological conservation – the development of a discipline
17 February Dr W. M. Bray and Dr I. C. Glover – Scientific investigation or cultural imperialism: British archaeology in the Third World
24 February P. J. Parr – Bloomsbury, Baghdad and beyond
3 March Dr T. W. Potter – The Institute and Roman archaeology: past, present and future – an outsider's view
10 March Professor B. W. Cunliffe – Britain and the Continent: changing perspectives in Iron Age research
April to July An exhibition in the University Library, Senate House, of the publications of eminent archaeologists associated with the Institute: Gordon Childe, Dame Kathleen Kenyon and Sir Mortimer Wheeler.
April in the British Museum An exhibition on the important Palaeolithic site at Boxgrove, Sussex, currently being excavated by the Institute's Field Unit.
29 April HRH The Princess Anne, Chancellor of the University, visited the Institute and unveiled a plaque in commemoration of the event (see p. 25 above). The date was the anniversary both of the formal opening of the Institute in 1937 (see p. 2 above) and the opening of the present building by HM Queen Elizabeth, the Queen Mother, in 1958 (see p. 23 above).
30 May Roman Army Day: a one-day Conference at the Institute that included illustrated papers by Professor John Wilkes on Excavations of the Roman out-post fort at Strageath, Perthshire; Peter Connolly on Roman arms and armour; Dr Margaret Roxan on Military diplomas and the order of battle; Dr Richard Reece on The military budget and its economic effects; Mark Hassall on Forts and frontiers.
The Ermine Street Guard presented a display in the gardens of Gordon Square.

- 20 June Jubilee Reunion Party for past and present staff, students and members of the Institute held in Gordon Square (heavy rain later in the evening forced it to adjourn into the Institute building).
- 6–10 July Conference on Recent Advances in the Conservation and Analysis of Artefacts with a reunion of all past and present staff and students of the Institute's Conservation Department.

SALUTATION
from the
INSTITUTE OF CLASSICAL STUDIES

*Te, Soror alma, minor natu germana saluto,
cui bis quina hodie claudere lustra datur.
te florere hodie, te tot viguisse per annos
gaudebit si cui classica cara manent;
nos tamen ante alios laudis tibi nectere sertam
convenit; hoc sociis non grave munus erit,
namque ambae ad metam pariter contendimus unam,
nota sit ut nostris gloria prisca virum.*

Ana Healey
Naomi Alvarez

John Barron
Alicia Totolos

The Contributors

Bryan Alvey, BA, MA, has extensive field experience in England, Scotland, Thailand, India and Ecuador. He took his first degree at Durham and his MA at the Institute of Archaeology, where he is currently working part-time for his PhD. He became interested in computers in 1982, and recently left his post at the Victoria and Albert Museum to establish Cultural Heritage Information Consultants, developing a 3D micro-based graphics system for field archaeology and computer systems for museums.

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Warwick Bray, MA, PhD, FSA, is Reader in Latin American Archaeology at the Institute of Archaeology. He is currently directing a multidisciplinary project in the Colombian Andes and carrying out research on the development and technology of native American metalworking.

Don Brothwell, BSc(Lond), MA(Cantab), is Reader in Zooarchaeology at the Institute of Archaeology. He was previously Head of Anthropology at the British Museum (Natural History), London. He was a founder editor of the *Journal of Archaeological Science* and a founder of the Association for Environmental Archaeology; he is currently an editor of the Cambridge Manuals in Archaeology. His current research includes aspects of human palaeobiology, avian zooarchaeology and mammals palaeopathology.

Peter A. Clayton, FLA, FSA, FRNS, is an Honorary Member of the Institute of Archaeology. Formerly Managing Editor of British Museum Publications and a Director of B.A. Seaby Publications Ltd. Currently freelance Editorial Consultant and lecturer in Egyptology and numismatics. Research interests are Egyptian Royal ushabti figures and the early history of Egyptology, both fields where he has published.

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Angela Croome, MA, was born in Peking and became interested in flying machines and science during service with the WRNS in the last war. She won a scholarship to Somerville College, Oxford, where she read English Language and Literature. Subsequently, as a science journalist, she travelled extensively in Europe, the USA and the Soviet Union, with especial reference to nautical archaeology. Her present commitments are concerned with further reforming the law relating to wrecks. She has been Reviews Editor of the *International Journal of Nautical Archaeology* since its inception.

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Barry Cunliffe, MA, PhD, DLitt, FBA, FSA, is Professor of European Archaeology in the University of Oxford. He has excavated widely in Britain at Bath, Fishbourne, Danebury, Hengistbury, etc., and has been involved in organising the British excavations at Carthage. His especial interests and current research projects lie in the Iron Age and its social aspects.

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Peter L. Drewett, B.Sc., Ph.D., F.S.A., M.I.F.A., is Lecturer in Prehistory and Archaeological Field Techniques at the Institute of Archaeology and Director of its Field Archaeology Unit. He is currently studying aspects of landscape archaeology on the South Downs and the Preseli Mountains. He is also undertaking a survey of Prehistoric Barbados for the Barbados Museum.

John D. Evans, MA, PhD, LittD, DHC(Lyon 2), FBA, FSA, is Director of the Institute of Archaeology and Professor of Archaeology, formerly Professor of Prehistoric Archaeology at the Institute. His research interests are mainly in Mediterranean prehistory, island archaeology, and the history of archaeology. Lately President of the Society of Antiquaries of London.

Ian Glover, BA, PhD, is a Lecturer in Prehistoric Archaeology of South and South East Asia at the Institute of Archaeology. He studied in Sydney and Canberra, Australia, and has undertaken excavations and surveys in Australia, Timor, Sulawesi and Sumatra in Indonesia, and Thailand since 1980. He has research interests in hunter-gatherers and early agriculture, ethnoarchaeology, trade and state formation. He is an editor of *World Archaeology*.

Ian Hodder, MA, PhD, FSA, is Lecturer in the Department of Archaeology, University of Cambridge. He took his first degree at the Institute of Archaeology in 1971, then did research at Cambridge. Having taught at Leeds University, he returned to Cambridge to his present post. His first

publications were concerned with spatial analysis in archaeology and he has more recently been concerned with symbolic and interpretive archaeology.

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Rick Jones, BA, PhD, FSA, has been Lecturer in Archaeology in the School of Archaeological Sciences at the University of Bradford since 1978. After studying Ancient History and Archaeology at Manchester University, he was a research student at the Institute from 1973–75, working for his PhD on Roman burial. He has researched and directed excavations in Spain and France as well as in Britain, particularly at Binchester Roman fort and on rural sites in the hinterland of York. His research interests include burial archaeology, urbanisation, frontier studies and the analytical study of pottery.

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T. W. Potter, MA, PhD, FSA, is Assistant Keeper in charge of Roman Britain at the British Museum. Apart from Romano-British archaeology, he also works extensively in the Mediterranean, especially in Italy, where he is currently excavating a Roman villa and medieval settlement near Rome.

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Mark Redknapp, BA, PhD, FSA, MIFA, is presently Assistant Curator (Archaeology and Local History Section) at the Passmore Edwards Museum. He studied for his first degree at the Institute and subsequently obtained his doctorate for a thesis on the Roman and medieval pottery industries of the Oest German Eifel. A former President of the Institute of Archaeology Underwater Research Group, he was a founder member of the Marine Archaeology Survey (MAS) and has been its Chairman since 1985. He has directed excavations in England, Scotland and Germany and has been involved in underwater archaeology since 1976.

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Richard Reece, BSc, PhD, FSA, FRNS, is Senior Lecturer in Roman Archaeology at the Institute of Archaeology. His first degree was in Biochemistry at University College London, he then took a diploma in education and taught chemistry for six years. His particular interests are in Roman coins and pottery; Roman Britain, from an anti-classical point of view, and the Later Roman Empire from an anti-British point of view.

Nigel J. Seeley, BSc, PhD, CChem, FRSC, FIIC, FSA, is Head of the Department of Archaeological Conservation and Materials Science at the Institute of Archaeology. His research interests relate to the inorganic materials used in antiquity, including their technology, deterioration and conservation.

Gerry Talbot, MA, ALA, joined the Institute of Archaeology in 1946 and succeeded Joan du Plat Taylor as Librarian, retiring in 1975. She has excavated with Sinclair Hood at Knossos and Chios, with Kathleen Kenyon at Jericho and Jerusalem, with Peter Parr at Petra and with Chrystal Bennett at Buseira and Tawilan.

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Isobel Thompson, MA, PhD, studied for her doctorate (on Late Iron Age pottery in south-eastern England) at the Institute whilst a member of the library staff. She left in 1984 to divide her time between domestic matters and freelance pottery reports, indexing for the Society of Antiquaries, editing part-time work for the Institute Summer Schools, and setting up a cataloguing system for a new museum in Harrow, London.

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University College London

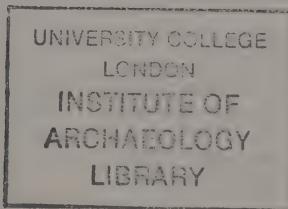
UNIVERSITY OF LONDON

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BULLETIN

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Review Supplement



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LONGWORTH, Ian and CHERRY, John (eds.).

Archaeology in Britain since 1945: new directions. With contributions by Nick Ashton et al. London, published for the Trustees of the British Museum by British Museum Publications, 1986. 248 pp., 133 figs. £12.50 (£9.50 to visitors to the exhibition).

This book was designed to accompany the British Museum's 1986 exhibition of the same name. The exhibition concentrated on the archaeological contribution to our knowledge of prehistoric, Roman, medieval and post-medieval Britain, illustrating the advances which have been made since the Second World War. The Institute of Archaeology can be said to have played a formative part in these advances, and indeed in the exhibition itself, for 'towards the end of the Second World War an *ad hoc* meeting of some 280 archaeologists held at the Institute of Archaeology drew up a plan for the future of the subject in Britain' (p.7). Thus it is appropriate that the 'book of the exhibition' should be reviewed in these pages.

The book does not pretend to be a catalogue of the exhibits, although much that is discussed or illustrated here did appear in the museum showcases or videos. It is intended as a summary of the present state of knowledge and an introduction to British archaeology for students and the general reader. It is made up of five chapters, all but one written by staff of the British Museum itself. Prehistoric Britain is covered by Ian Longworth, Nick Ashton and Valerie Rigby; Roman Britain by Tim Potter; Anglo-Saxon England by Leslie Webster; Technology, Towns, Castles and Churches by John Cherry; and the Medieval Countryside by John Hurst (the one non-British Museum author). There is also a short glossary, a select bibliography, and a comprehensive index.

Each chapter is prefaced by a map of sites mentioned in the text, and these immediately raise one important point. Is the very title of the book accurate? Are we being presented with a survey of archaeology in Britain, or of archaeology in England (and largely southern England at that)? Is the imbalance which the following few simple statistics will show a true reflection of the country as a whole in the respective periods? Is it a reflection of archaeological endeavour

since the war? Is it a reflection of the interests of the individual authors? The editors' introduction tells us that what we shall be reading is 'a personal selection of what to them [the authors] seem the most important achievements within the periods they cover' (p.11). Inevitably, an ambitious scheme such as a book of this kind must involve some selection and much omission, but does it really need to be as drastic as this?

Let us consider the maps, then. That for the prehistoric period shows 124 sites including six in Wales, twelve in Scotland and the Northern Isles, and five in Ireland (both north and south). The chapter entitled 'A Roman province' shows 73 sites, thirteen on or north of Hadrian's Wall (including Carlisle) and seven in Wales. Chapter 3 deals with 'Anglo-Saxon England AD 400-1100' so its parameters are clearly stated, but where can we go for information about the 'Dark Age' sites in Wales or Scotland so ably excavated by Leslie Alcock, among others? Are the northern and western areas of Britain to be cast forever as the 'Celtic fringe'? Where are Dinas Powys, Dunadd and the Irish sites of this period? Chapter 4 is slightly more wide-ranging in that Wales and Scotland together score six points on the map, but Ireland has fallen away totally; the same is true of the final chapter where even Scotland and Wales have disappeared. I can't help feeling that some archaeological colleagues to the north and west must be feeling somewhat neglected.

Still, these omissions must be attributed to the personal choice of the authors, as mentioned above, and I have perhaps pursued the matter too far. Nevertheless, I should like to see equivalent exhibitions and books from Welsh, Scottish and Irish museums. Where would the emphasis then fall?

For the rest, the book does fulfil its stated aims despite its geographical bias. The chapters cover their topics briskly and competently, although much of the subject matter has been published elsewhere. There is little that is new in this volume, but it has the virtue of bringing together in easily digestible form a considerable body of archaeological evidence which should help the general reader (and perhaps the aspiring applicant for a university place) to gain an impression of what archaeology has achieved over the past 40 years. A special mention must be made of the excellent drawings and

reconstructions by Simon James which bring the whole subject to life in a most exciting way.

This volume can do no more than skim the surface of a highly complex subject; its select bibliography indicates the next step for the reader, but in itself has to be regarded, like the book as a whole, as a sign-post rather than a goal.

HELEN CLARKE

MIKET, Roger and BURGESS, Colin (eds.).
Between and beyond the Walls: essays on the prehistory and history of North Britain in honour of George Jobey.
Edinburgh, John Donald, 1984. xii + 424 pp., illus. £25.00.

George Jobey will be known as the leading prehistorian and field worker among the Romanists of Northumbria and Durham. This book is a great tribute to a distinguished academic and teacher. While I generally disagree with the idea of *Festschriften*, this is one of the exceptions and shows clearly the great esteem of his colleagues for his work.

The twenty contributions presented here are by the specialists who have at one time or another worked with him. The subject matter ranges from Folk Medicine to Celtic Art; from Neolithic long cairns to the techniques of aerial photography. The only thing that they have in common is geography. So it comprises a collection of papers on Jobey's stamping ground mainly up to the Roman period. For anybody wanting a general introduction to work done over the last 40 years in North Britain between the walls this is an excellent work and it has been well edited.

R. F. TYLECOTE

STEAD, I. M., BOURKE, J. B. and BROTHWELL, Don. *Lindow Man: the body in the bog.* London, published for the Trustees of the British Museum by British Museum Publications, 1986. 208 pp., 55 figs., 2 pls. £15.00.

BROTHWELL, Don. *The bog man and the archaeology of people.* London, published for the Trustees of the British Museum by British Museum Publications, 1986. 128 pp., illus. £5.95.

A good deal of publicity surrounded the find, in 1983, of the body of a young adult man, preserved in the acid peat of a raised bog at Lindow Moss in Cheshire, England. Such finds have been made in many parts of northwestern Europe but, apart from the famous Danish bog bodies, very few have been conserved and still fewer have been studied in detail. The British Museum took advantage of Lindow Man's discovery to mount a wide-ranging investigation which involved not only archaeologists, but also anthropologists, pathologists, anatomists, human biologists, dentists, biochemists, photogrammetrists, botanists, palynologists, conservators, chemists, forensic scientists, materials scientists, radiologists, physicists, zoologists, microbiologists and even an oculist. *Lindow Man* is the formal account of this project. It contains many specialist reports, arranged into four sections. Section I (eleven chapters) deals with the discovery of the body, its excavation, conservation, recording, dating and reconstruction. Biological reports on the body itself are in Section II (eight chapters), including anatomy, radiography, pathology, biochemistry and forensic investigations. Section III (ten chapters) covers pollen analysis and macrofossil evidence from the peat sequence within which the body was found, as well as investigations of the gut contents. The four chapters of Section IV place Lindow Man in his archaeological context, with a survey of bog body finds in Europe, discussion of folklore and a short final summary of the results. At the end of the book is a useful gazetteer of bog body finds in Britain and Ireland.

Section I opens with the discovery of the body during commercial peat cutting operations and its removal, encased in a block of peat. The actual excavation of the body itself was carried out under laboratory

conditions by the British Museum, where it could be kept refrigerated. Details are given of the procedures used to prevent deterioration of the body during its examination, as well as the final conservation measures. A freeze-drying process was employed, resulting in very little shrinkage. Dating the death and burial of Lindow Man proved to be a real problem and the efforts of three separate radiocarbon laboratories (Oxford, British Museum and Harwell) are recorded in three chapters. Samples of bone and soft tissues, taken from the body itself, produced different results at different laboratories. Samples of the peat encasing the body yielded dates which differed yet again. This concentration of several laboratories on one project is unusual and their discussions of possible sources of error make interesting reading, although the differences are not yet resolved. The external form of the body was recorded by photogrammetry and Section I includes a description of the methods used, leading up to what must be a first for archaeology - contour maps of the skin surface of a bog body. This section also includes an account of the reconstruction of Lindow Man's facial features, and describes the two artefacts discovered on the body - a fox fur arm band and a cord, tightly knotted around the neck, collar size twelve and a half!

No stone was left unturned in the investigation of the body itself, covered in the second section of the book. Its skin was analysed for traces of woad or tatooing, its hair for dye, and washings from the skin surface were examined for remains of insects. Scanning electron microscopy was used to examine the cut ends of hair and nails. Various methods of non-invasive examination were also used; magnetic resonance images were made along with two types of radiography - xeroradiography and computed tomography. Both radiographic methods have been applied successfully to the dried tissues of Egyptian and other mummies, but in Lindow Man computed tomography proved more successful at showing detail of the decalcified bones which resulted from burial in acid peat. The skin was, in fact, the best preserved and most robust part of the remains, although muscle and other soft internal tissues were well preserved in places. Whilst the outline of the skin remained intact, its cell structure had been lost and most of its bulk was made up of fibres of collagen (a protein which is one of the major building blocks of the body). These, however, showed all of the microscopic

features of fresh collagen and chemical analysis identified several compounds which would normally be expected to be lost rapidly in the process of decomposition. The medical and forensic chapters are of particular macabre interest, because they show that, while Lindow Man had been generally in good health, he met his death violently. Severe wounds on top of his head would eventually have killed him, but did not, apparently do so immediately. His neck had also been broken, presumably by the cord tied around it, which seems to have been tightened by twisting with a stick. In addition, there was a wound just above the cord in the area of the jugular vein. The pathological and forensic details make fascinating reading.

Pollen analysis, plant macrofossils, and the remains of beetles, water fleas and midges from the peat surrounding the body all suggest that the surface of Lindow Moss was characterised by peaty pools at the time of the killing. Indeed, the body seems to have been pitched face downwards in one of the pools. Inside the body cavity, the stomach and parts of the small intestine survived, yielding some 20g of contents which were investigated for plant and animal remains. All the details of identification and their interpretation are given in Section III. The last meal seems to have been a wheat and barley bread - bread rather than gruel, because electron spin resonance spectroscopy showed that chaff in the mixture had been heated to 200-250°C. Also identified inside the gut were a number of eggs from parasitic nematode worms - whip-worm and maw worm, although it was not possible to suggest the severity of Lindow Man's parasite infestation.

The bulk of the chapters in the book are concise, separate reports which, although they do refer to one another, are not strongly linked together. They vary a great deal in their breadth of treatment. Some are general descriptions or surveys of literature whilst others are attempts to answer very specific questions. Several suggest that they are the results of preliminary investigations only and it is clear that work on Lindow Man will continue. One way in which the book will make its mark is that it establishes techniques which will be very useful in future. If another bog body is found, it should be much easier to plan its investigation as a result.

Whilst *Lindow Man* is very much a technical report, not really written for the general reader, *The Bog Man* is very much the opposite. It is an account of the Lindow Man project in non-technical terms, which places it in the context of other finds, explains techniques and has many illustrations. The findings are organised into six chapters, much along the lines of sections in the *Lindow Man* book, but each summarises the complex evidence and draws on comparative material from other bog bodies, mummies and so on. There is a chapter on the discovery, excavation and conservation, one on the forensic aspects, another on the reconstruction of Lindow Man in life, and others on the pathological investigation, environmental context and examination of the gut contents. A seventh chapter puts Lindow Man into context by describing a wide selection of preserved ancient bodies from all corners of the world. It deals with other European bog bodies, remains from the frozen tombs of the Altai mountains in southern Siberia, naturally freeze-dried bodies from Greenland, Egyptian mummies and the remarkably preserved bodies of Chinese nobility, as well as the mummies of the New World, from the Aleutian Islands to the Andes. *The Bog Man* is a useful introduction for the non-specialist reader. The Lindow Man find has aroused a great deal of public interest and the book will no doubt attract a wide readership.

SIMON W. HILLSON

CUNLIFFE, B. *Danebury: an Iron Age hillfort in Hampshire*. Vol. 1. *The excavations, 1969-1978: the site*. Vol. 2. *The excavations, 1969-1978: the finds* (CBA Research Report 52). Nottingham, Council for British Archaeology, 1984. vii + 199 pp., 135 illus., 56 pls., 7 micro-fiches (Vol. 1); ix + 337 pp., 224 illus., 10 microfiches (Vol. 2). £40.00.

CUNLIFFE, B. *Danebury: Anatomy of an Iron Age Hillfort*. London, Batsford, 1983 (hardback), 1986 (paperback). 192 pp., 100 illus. & £14.95 (hardback), £9.95 (paperback).

These publications provide two accounts of the first stage of Cunliffe's Danebury project (1969-1978). Excavation has continued at Danebury since 1978 and some later results are included, notably the plan of additional areas cleared in the west of the site, but detailed discussion refers to the work completed by 1978. The CBA publication represents in Cunliffe's own words 'the definitive report of the first ten years' work'; the Batsford volume summarises this work for a wider audience. Each publication, in its different way, represents a major achievement in communication.

The Danebury excavations constitute one of the main field projects to take place in Britain since Wheeler's campaigns of the 1930's. Cunliffe sees the intervening period as one which produced many major problems, but answered few, and he sets the stage thus for the late 1960's: 'It was extremely difficult to move on to more interesting topics like social structure, population dynamics and political change. We had come to a threshold of knowledge beyond which were only fairy stories of what might have been'. It appeared that large scale area excavation inside a Wessex hillfort would break the impasse: 'But in the large-scale excavations that were just beginning, and the designing of broadly based research strategies for gathering data, lay the hope of being able to cross that threshold. It was against this background that the excavation of Danebury was planned'.

The results which Cunliffe is able to present significantly advance Iron Age studies in Britain and a whole body of fresh evidence has been made available promptly and clearly. Certain categories of finds are especially revealing, notably the plant and animal remains and ironwork, and some specialist reports represent major pieces of research in themselves: Martin Jones on plants, Annie Grant on animal husbandry, Clive Orton on statistical aspects of radiocarbon dating, to cite just three of the skilled band of collaborators that Cunliffe managed to enrol. The relatively abundant human remains too, dealt with by Bari Hooper, represent an important and surprising series of finds which must take a preponderant role in relating Iron Age Wessex to a broader Iron Age context.

But Danebury is of interest beyond the confines of the Iron Age since it represents a serious attempt to tackle a large scale

excavation using state of the art techniques of investigation, recording and interpretation. As such, Danebury may serve as a commentary on the effectiveness of current approaches to solve problems of social structure and dynamics, as Cunliffe spelled them out.

Cunliffe himself makes the point that archaeology has developed greatly during the life of the Danebury project and that his own methods and approach have changed since the first Danebury campaigns were designed. Indeed, it was only by experimenting with a project on this scale that problems of procedure would be distinguished and tackled. The most resistant problem that has come to the fore is the general one of the difficulty of dealing with large quantities of prehistoric data that do not lend themselves to the kind of mechanical classification that may serve later, industrial remains. If the function of features and areas of a site and so of the site as a whole are to be understood in detail, contemporaneity or near contemporaneity of related features must be established, but this requires study of fragmented and refitted material (for the Iron Age pottery) and the working out of relative chronologies at a level of detail that is likely to appear taxing for any large scale excavation.

Certainly, the experiment at Danebury has demonstrated the need for such detailed studies, and the inability of radiocarbon dating to provide an adequate substitute for a carefully engineered archaeological chronology based on stratigraphy, refitting and seriation (of stylistic as distinct from functional or technological traits; ie. for pottery, decoration rather than fabric). A further myth laid to rest by demonstration is the widespread belief that comprehensive evidence from prehistoric settlements is more readily translatable into social structure than evidence from prehistoric cemeteries.

Like Maiden Castle, Danebury has raised many problems for future resolution, but at a level of detail that was unimaginable in Britain in the 1930's. One such key problem that could only be raised by meticulous and comprehensive research involves the evidence for subsistence: both floral and faunal assemblages are shown to remain virtually constant over the whole life of the settlement, perhaps for half a millennium, and yet this is a time when demographic and commercial changes are thought to have taken place

on a large scale.

The Danebury project may already be perceived as a magnificent achievement and it is certain to be regarded in the history of British field archaeology as a landmark for this generation as Maiden Castle was for the last. These reviewed volumes provide a fitting commentary on this significant enterprise.

F. ROY HODSON

STEAD, I. M. and RIGBY, Valery. *Baldock: the excavation of a Roman and pre-Roman settlement, 1968-1972* (Britannia Monograph Series, no. 7). London, produced for the Society for the Promotion of Roman Studies by Alan Sutton, 1986. 435 pp., 160 figs., 55 tables. £23.00 (incl. postage).

Baldock should go down in the publishing history of archaeology as a *cause célèbre*. Like many such cases it may be that posterity will regard it simply as a good site report and wonder why it should be subject of particular remark. The reason, which needs putting on record, is that it marked the turning point of the battle against the senseless use of microfiche and the victory of communication over bureaucratic policy.

The years between 1968 and 1972 marked a period of house building and infilling at Baldock in Hertfordshire and the area under threat had previously turned up a number of Iron Age and Roman burials and other remains. This was not a well known walled town of Roman Britain but an area of pre-Roman settlement which continued as a 'small town' into and through the Roman period. Structures on the site were mostly in the form of pits, gullies and ditches, there were no complete stone buildings and certainly no mosaics or sheets of painted wall plaster; in one way this was one of the best reasons for the extensive and lengthy excavations, for we know so little in detail about such sites. In the final report the site and

structures take up 50 pages, the burials 30 pages, miscellaneous finds about 107 pages, and the pottery nearly 200 pages.

If, as was suggested, the publication guidelines had been followed, the ditches and pits, obviously far less important than mosaics and town walls, could be summarised in 40 pages or so, and then if the whole site could be compressed into that small compass the pottery could be tucked away in microfiche and the whole thing could slip almost unnoticed into a county journal. This was to reckon without an excellent excavator, with inside knowledge of the corridors of archaeological power, and an equally excellent pottery specialist who had made of the pottery report something much more than might be expected from such a site. They fought, and this publication shows that they won, and that it was worth winning. Ian Stead's site report is clear and an excellent summary of the sequence on the site. Val Rigby's pottery report is very good indeed and will form a sound basis for other people's work, especially on the first century AD.

Publication guidelines are made for the mediocre norm and so long as they are used sensibly on the nondescript they can cause little harm. But excellence is a rule to itself; Val Rigby's pottery report is excellent; thank goodness it has been well published and the little grey men on committees have for once bitten the dust.

RICHARD REECE

MILES, David (ed.). *Archaeology at Barton Court Farm, Abingdon, Oxon.* (Oxford Archaeological Reports, 3; CBA Research Report, 50). Oxford, published by the Oxford Archaeological Unit for the Council for British Archaeology, 1986. xiv + 66 pp., 33 figs. Microfiches in pockets. £19.75.

After being run out of Baldock (see review above), the little grey men seem to have roosted in Barton Court Farm. But it would

be unfair to let the mechanics of publication count too heavily against David Miles and the Oxford Unit, so I will relegate a horror of the text provided to a material polemical coda.

In 66 pages - including bibliography but excluding 4 introductory pages spread over 8 sides - we are given an introduction to, and summary of, 631 frames on 9 microfiches which contain the full excavation report. 'It is hoped (p.xii) that by publishing in this way we can provide detailed information as cheaply and flexibly as possible'. Grrrr... Down boy, down. Microfiche 1 is the 'printed element', ie. a microfiche of the printed book. A pity the book goes viii, ix, x, while the microfiche has viii, x, xi. Fiche 2 has 14 frames on, fiche 6, 21. In the printed text we are given the geographical and archaeological background (1-3), the site (4-19), landscape and resources (20-26), nuclei of ancient farming (27-37), towards a model of the villa estate by Martin Jones (38-42), communications, prosperity and trade (43-48), and change and continuity (49-54).

The site consisted of an Iron Age farmstead of the 1st century BC - 1st century AD, and a group of wooden buildings of the 5th-6th century. We are well guided through these remains and it would have needed only ten further carefully chosen printed pages to give us the basic evidence on which the dating depends. If the report was to be published cheaply and quickly that could have made an interesting 25-page (HBMC subsidised) article which would have been fully accessible in a local or national journal. One point here which does need further thought is the added cellar with the tessellated floor; I find it very difficult to believe that this was built for food storage. 'Nuclei' is interesting but diffuse and the same can be said for 'Communication and trade'. My worries centre on the large number of wide statements which are not backed up in microfiche, but describe a wide area over a long time with only the occasional reference. Martin Jones on the villa estate is useful so long as his map is disregarded and his acreages are understood as 'land, not necessarily in blocks but somewhere around, and within reach of, the farm'. 'Change and Continuity' is presumably the reason for making this report a full-scale book, for few sites show establishments from Iron Age to Saxon, but this chapter suffers badly from a failure to grow out of the hard

facts which ought to have been its base so that it reads like a selection of beautiful thoughts based on a very odd selection of random references. I hope future generations will find it odd to refer to the Belgic 'invasion' as an 'archaeological cliché' (p.49), while 'Saxon settlers' roam through the pages unopposed.

Some, but not all, of these criticisms grow from the divorce of the printed beautiful thoughts from the hard fact of microfiche. This did not happen at Baldock (see above); need it have happened at BCF? In the Baldock report the Roman Society provided 435 printed pages for £12.00, about 2.76p per page. BCF complete takes up 631 frames of microfiche. When 66 pages were printed by the CBA and added to the 9 microfiches the cost was £19.75. If all the 631 pages had been printed by *Britannia* monographs instead of being microfiched it must be assumed that the cost would have been 631 x 2.76p, or just under £17.50. Is overcharging by £2.25 for the inconvenience of microfiche really providing 'detailed information as cheaply and flexibly as possible'??

RICHARD REECE

military history from the time of Caesar to 'the groans of the Britons', religion (including Christianity), commerce, local government, and family life. There is a short bibliography, and useful indexes. Over 500 items are featured, some well known, others less so. We learn not merely about victories and conspiracies but also about British hunting dogs and hangover-inducing local beers. Inscriptions on stones and coins take their place alongside lead curse-tablets, oculists' stamps and graffiti; the British section of the *Notitia Dignitatum* makes a welcome appearance.

There is no space here to object to the inclusion or exclusion of particular items, or query the translations themselves. The editor is more at home with literary than with epigraphic texts, which are often accorded a more precise significance than their content warrants (eg. nos. 101, 111, 113). The deliberate decision to exclude Caesar's own account of his expedition to Britain, and Tacitus' version of Agricola's life, career and campaigns, in favour of derivative accounts and other less familiar material may not be accepted by readers who are forced to look elsewhere for some major texts they could have expected to encounter here. Perhaps if a second edition is proposed, Agricola could receive more of his due, space being found (for example) by reducing the section on Druids (much of which is, strictly speaking, unrelated to Britain). Note also that one inscription appears twice (nos. 132, 296) with slightly differing translations offered; the texts of 136 and 137 are identical. It is strange that the author retains the now discredited titulature of Cogidubnus, as *rex et legatus Augusti in Britannia* (no. 444), a titulature which is later used (p.227) to suggest senatorial rank for Britain's favourite client king. In general the presentation is clear. However, it is hard sometimes to distinguish between the texts themselves and the author's explanatory remarks, especially when a page is turned; the latter might have gone into italics and been merged with the footnotes which are irritatingly appended to each chapter. Often it is unclear whether an item is on stone, lead or pottery; or whether the original Greek or Latin text was prose or poetry. Sometimes the provenance of an inscription, eg. Castlehill (nos. 136, 137, 357), Trecastle Hill (no. 203), or Bollinhope Common (no. 382) will mean little to the reader seeking a geographical context for the document. It is easy to cavil.

IRELAND, S. *Roman Britain: a sourcebook*.
Beckenham, Croom Helm, 1986. 266 pp.,
illus. £18.95 hardback; £9.95 paper-
back.

For those who teach courses on Roman Britain, or as students require easy access to source material alluded to in the mainstream textbooks, this handy compendium, encompassing literary, epigraphic and numismatic material, will be especially welcome. This is in effect a companion to the invaluable (but here unmentioned) collection by R. W. Moore, *The Roman in Britain: A Selection of Latin Texts* (3rd ed., 1954). After an introductory chapter which firmly warns the reader against too uncritical an acceptance of what are often chance survivals of evidence, sections deal with the geography and peoples of Britain, the political and

Better to applaud this helpful collection, which will find a home on many bookshelves, yet be unlikely to gather much dust there.

LAWRENCE KEPPIE

cohesion. This problem does not arise at the initial presentation and there is a case for stronger editorial control at the publication stage. It is at this point that details of specific sites and NGR's could be added to great advantage. The only author to provide the latter is Gordon Maxwell in his succinctly written account of the evidence from the Roman period.

The book is published by John Donald and is one of the successful *Scottish History and Culture Paperback* series which covers many lesser known subjects and is available at relatively low cost.

JUDITH HARRIS

FENTON, Alexander and STELL, George (eds.).
Loads and roads in Scotland and beyond: road transport over 6000 years. Edinburgh, John Donald, 1984. vii + 144 pp., 61 figs. £8.50.

Routes of communication must surely lie at the heart of the growth and development of every society and, as Professor Coles points out, the character and extent of a country's roads provide a mirror of its organisation and its central control - or lack of it. Because of the inherent problems these routes are rarely studied by the archaeologist. Instead there is a temptation to draw lines on maps between the known sites and artefact distributions with scant regard for the practical aspects involved.

Loads and roads in Scotland and beyond was originally presented to a one day conference at the Society of Antiquaries of Scotland. The 'and beyond' refers in particular to Professor Coles' work on the Somerset Levels and to ethnographic evidence in both of Alexander Fenton's valuable contributions. The papers cover problems concerning the identification and dating of communication routes and take into account the initial stimuli - social, economic or military. Questions are also raised involving the levels of technology displayed, not only of the constructions themselves but of the traffic which was subsequently to use them. The historian, geographer, student of place names, engineer and archaeologist can all contribute but unfortunately few of the individual papers use more than two of these sources.

As with many published conference proceedings, while each paper is stimulating and adds much to this little-studied topic, the overall impression is one of lack of

ROYAL COMMISSION ON ANCIENT AND HISTORIC MONUMENTS IN WALES. *Inventory of the ancient monuments in Glamorgan I: Pre-Norman. Part II The Iron Age and the Roman occupation.* Cardiff, HMSO, 1976. xx + 135 pp., 61 figs., 14 pls. £9.50.

This book, in the new-style format of period-based county publications, contains a great deal of useful information. As with all RCHM volumes, the quality of the illustrations is high, the individual site entries are detailed and now use metric measurements. The distribution maps with the coloured physical background are extremely useful. New features include arrangement by monument type rather than parish, and omission of isolated finds. The staff and their responsibilities are outlined (p.xvi), a great advance which gives credit to the Royal Commission investigating staff who for so long had to be anonymous in their writings for the inventories.

Glamorgan has received considerable attention from excavators in the past, and this has provided a useful dating framework for many categories of site. The area is particularly fortunate in the Welsh context because it is ceramic in the Iron Age and relatively rich in material culture in the Roman period. This is, after all, the only

part of Wales with a group of villas, some of them quite magnificent.

The small non-defensive enclosed settlements have generally been assigned to the Iron Age section, although no doubt many belong also to the Roman period. Continuity of settlement beyond the conquest may be common in this area since many sites, promontory forts for example, produce Roman pottery. Without more extensive modern excavations, it is not clear whether this represents continuous use or just re-occupation. The researcher interested in the Iron Age would be advised to check through the Roman settlements, and vice versa because of the difficulty in allocating sites to one chapter or another.

Presumably many more small enclosures will be found through aerial photography, and so this part of the book will no doubt quickly become out of date. For more substantial forts, the listings will long have reference value. The discussion sections highlight the most important features of the Glamorgan sites and offer some interpretation; the data offer far greater potential than has been exploited here, and it is hoped that this will be seized by the Commission staff in other works, and by others. All those interested in the periods, and the interaction between Roman and native will find this an extremely useful volume, and all libraries should purchase it. The new format is a welcome advance, and places the book within the reach of period-based specialists.

HAROLD MYTUM

have often felt able to neglect such data as can be found, or use models from other parts of the world with very different environmental conditions from Western Europe. Fenton has written a series of papers which make a contribution to filling this gap.

These two volumes contain papers written over the last 20 years, many originally published in ethnographic journals from East Europe not readily available here. Five papers in Volume 1 are on theoretical aspects of regional ethnology; the remainder in Volume 1 and those in Volume 2 are concerned with ethnological evidence of material culture. Though many of the papers relate directly to Scottish experience, they are of general relevance to pre-industrial life in western Europe.

In the first book there are papers on house building styles, the use of cattle dung as fuel and the tools and methods of reaping and threshing. The second volume has only six papers: on yoke types, draught oxen, manuring with seaweed, paring and burning, and two on the use of turf and peat for fuel. Descriptions of farming processes such as these contribute to the building of theories about early agricultural practices in temperate Europe. They are a reminder that practices such as the use of seaweed for manure or of dung for fuel may have been locally important for successful agriculture. The descriptions of crop processing methods raise interesting questions about the origin of charred plant remains on archaeological sites and may be valuable for the understanding of site formation, especially as recent ethnographic work in this field has been done mainly in the Eastern Mediterranean.

Where Fenton is not recording his own observations of surviving practices, he uses early historical accounts. In parts of Scotland these are often no earlier than the Old and the New Statistical Accounts of each parish, which were compiled at the end of the eighteenth century. They depict societies already very different from prehistoric and early historic times: the potato was by then an important crop, and animal husbandry was geared to supplying the urban markets of England and Scotland. Though ethnographers, like historians, have sometimes been inclined to assume that the earliest written account of an activity describes an earlier unvarying practice, Fenton mostly avoids this assumption. He is familiar with, and uses, some of the archaeological evidence, parti-

FENTON, Alexander. *The shape of the past: essays in Scottish ethnology* (Scottish history and culture paperbacks). 2 vols. Edinburgh, John Donald, 1985-6. viii + 191 pp., 45 figs.; vii + 184 pp., 57 figs. £12.10; £12.00.

Ethnographic data for western Europe is very poor compared with other parts of the world, and prehistorians working in Britain

cularly for the Viking world. Not only those concerned with early farming in western Europe but any archaeologist working on prehistoric British sites will find something of value in these papers.

DALE SERJEANTSON

HARVEY, Nigel. *Historic farm buildings study: sources of information*. London, Ministry of Agriculture, Fisheries and Food, 1985. vii + 76 pp. Spiral binding. £5.00.

This booklet has been produced by the Ministry of Agriculture, Food and Fisheries principally for the use of its own staff who may be called upon to advise on the re-use of historic farm buildings. The booklet begins with an introduction to the development of Historic Farm Buildings studies and the importance of their sympathetic conservation.

An Historic Farm Building is defined as a farm building along with its ancillary machinery but excludes farm houses, mills, commercial buildings and tithe barns. A date limit at the turn of this century has been imposed for buildings in this study.

The sources of information are directed at the recording and study of individual farm buildings rather than the whole farm complex. This approach is not only restricted to Historic Farm Buildings and still needs to be satisfactorily resolved in relation to the recording of all monuments in their historical and topographical context.

The scope for the study of Historic Farm Buildings is enormous; it is estimated that there are at least 600,000 in England and Wales alone. The majority of these buildings still earn their living as they comprise about one quarter of the farm building stock.

The earliest surviving structures tend to be barns built prior to the middle of the 18th century - of which there are 15-20,000. Few other types of structures survive prior to this date. After the middle of the 18th

century an increasingly wide range of structures are found to survive.

The booklet provides lists of the relevant national bodies with an interest in Historic Farm Buildings. It also catalogues national and local surveys of Historic Farm Buildings. Published sources are listed as well as less easily available material contained in theses and locally published booklets. A useful introductory reading list has also been provided.

It is unfortunate that this booklet was not produced with a more substantial binding, as it will remain an invaluable source for anyone engaged in the study or conservation of Historic Farm Buildings. It should also have a wider appeal to all those involved in the study of all aspects of post-medieval agriculture. The publication comes at a time when government prompted diversification in the countryside may see Historic Farm Buildings being viewed as a tourist resource rather than a liability as they often have been in the past.

W. D. COCROFT

MITCHELL, Stephen (ed.). *Armies and frontiers in Roman and Byzantine Anatolia: Proceedings of a colloquium held at University College, Swansea, in April 1981* (British Institute of Archaeology at Ankara Monograph 5) (BAR Int. Ser. 156). Oxford, British Archaeological Reports, 1983. iv + 378 pp., 22 figs., 39 plates. £16.00.

This volume is the result of a colloquium held in Swansea in 1981 that was intended to supplement discussion of the eastern frontiers which had been poorly represented at the series of Limeskongressen. Such was the success of the Swansea colloquium that a second was held at Sheffield in 1986 under the title 'The Defence of the Roman and Byzantine East' (BAR S 297), whilst a third is now in planning.

Anatolia has been kind to the archaeologist in the number of Roman and Byzantine remains, kind also to the historian for the wealth of documentation. The seventeen papers here bring us up to date with new discussions on well known problems as well as presenting us with new chapters in our knowledge through recent discoveries.

Speidel's paper opens the way by noting the enormity of epigraphic data from Anatolia and the plethora of studies that can arise from it. Indeed, such is this wealth that many papers here take their cue from recently discovered or newly interpreted inscriptions. Numismatic documentation is also discussed by Howgego. As is to be expected, historical geography looms large amongst the papers. An examination of ancient sources, modern travellers' accounts and discoveries combine to form the topics in a number of papers: Syme - a delightful discussion of a problem in Strabo; French, Lightfoot, Whitby, Howard-Johnston, and Saunders on a little known period in the history of Qalat Seman. There are a number of interesting historical papers: Mitchell on the impact of military economy in the area; Kettenhofen on the campaigns of Gordian III; an interesting paper by Hopwood on local policing; and Arvites on campaigns in the late eighth century. As noted by Mitchell in his introduction, archaeological evidence is not so well represented but there is an engaging account by Wagner on recent work in Osrhoenae. Participants at the colloquium will remember the fine photographic exhibition presented by Saunders and Sinclair; in his paper Sinclair relates this documentation to a discussion of some major architectural remains. A synthesis of archaeological and historical information is presented by Wagner through bringing our attention to the fine series of maps produced by the University of Tübingen.

However, throughout the volume the emphasis has been on groundwork in Anatolia. Such is the range of the papers here, and their full notation, that the interested student will find some excitement in almost every aspect of Roman and Byzantine Anatolia.

JULIAN BOWSHER

ZVELEBIL, M. *From forager to farmer in the Boreal Zone: reconstructing economic patterns through catchment analysis in prehistoric Finland* (BAR Int. Ser. 115). 2 vols. Oxford, British Archaeological Reports, 1981. 472 pp., figs., maps. £20.00.

The transition from a hunting-fishing to a farming economy has been an issue of long-standing interest for archaeological research in Northern Europe. In a strict and narrow sense it involves a change from relying upon the food resources available in northern habitats to producing food through stock-rearing and grain-cultivation of species which by and large are alien to the northern environment. That such a change was far from being an abrupt episode but involved a gradual, long-term adaptation consisting of intermediate stages, is generally well documented in archaeological record. But in a broader sense the transition from hunting and fishing to farming was more than a simple substitution of one economic strategy by another - it involved the radical transformation of a society's mode of living. Economic transformation was inextricably connected with a profound change in the technological, ritual, social and organisational structure of society. The order in which these changes occurred is still imperfectly understood.

From this perspective let us then consider the contribution of Zvelebil's book. The geographical area which forms the basis of his study covers south-west Finland (mainly the Kokemäenjoki basin) and is broadened, in the final chapter, to cover the whole of the north-east Baltic region. Archaeological evidence from the area spanning four millennia (4000-0 bc) is briefly outlined as well as the environmental changes which took place over the same period.

Due to the unfortunate combination of poor preservation conditions and a paucity of excavation, Zvelebil has very little actual faunal and floral data on which to base the main objective of his research - the analysis of economic change from c.4000 bc to 0 bc. Thus he has to resort to information obtained through the site catchment analysis. This involves detailed information on the environment potential of 44 settlement sites which between them cover the Combed Ware, Corded Ware, Kiukais and Bronze Age

groups. All information is clearly tabulated and presented in an appendix (App. 4) - which appears in a separate volume, greatly facilitating references between text and figures. This is an invaluable source of information against which future evidence can be compared and it will serve as a solid foundation for years to come.

The conclusions of Zvelebil's analysis are, however, controversial. Some of the patterns implied by the archaeological evidence - such as the use of coastal settlements in the summer and inland sites in the winter by the hunting-fishing Combed Ware and Jakarla groups - are plainly contradicted by the catchment analysis which suggests precisely the opposite. While Zvelebil argues that this problem cannot be solved without additional evidence, one has the impression that he is tacitly in favour of the latter.

The general economic change is seen in two main stages: the first stage (c.2800-2000 bc) involves an increase in specialisation in marine resources, predominantly seal hunting. This is seen as a direct result of increased population pressure along the coast, further manifested by increased mobility of the inland Corded Ware groups. The second stage (c.1300-500 bc) involves a further growth in population pressure and a depletion of the seal as a stable food resource, leading to the gradual - and, one feels, unwilling - adoption of a mixed farming economy during the Bronze Age.

While this view is in contrast with the opinions of many Northern scholars, our main criticism must surely rest upon the fact that Zvelebil's interpretation is based not on the analysis of archaeological evidence from different settlement sites but on their economic potential - the two surely cannot be thought of as synonymous. According to Zvelebil's argument the migrant farming/pastoral (in the traditional view) communities of the Corded Ware/Battle Axe complex appear to have made no contribution to the economic change; the social and organisational implications of this migrant movement into the south-east Baltic are not even considered.

Thus a momentous change in North European prehistory is presented in this book entirely in the context of environmental determinism; more specifically only one factor is seen to have induced this change - a depletion in the seal population. This is a narrow interpretation and it will undoubtedly

be tested against future evidence. It contrasts with the broader perception evident in much of this work which, in spite of these drawbacks, is an important contribution towards a broader understanding of the archaeological problems in Northern Europe.

MADGALENA S. MIDGLEY

BARKER, Graeme. *Prehistoric Farming in Europe* (New Studies in Archaeology). Cambridge University Press, 1985. xviii + 327 pp., 100 figs. £27.50 hardback; £9.95 paperback.

This book starts with two chapters in which the sources of evidence for early European farming and the resources and constraints within which it operated are set out. Europe is then dealt with in seven regions: the Mediterranean, the Balkans with the middle Danube and the Ukraine, the Alpine region, the continental lowlands, Atlantic Europe, Britain with Ireland, and Scandinavia with the eastern Baltic. After a summary of the climatic and soil conditions which governed the possibilities for farming, the account of each region covers mesolithic subsistence strategies, the evidence for early farming, and developments in husbandry up to the beginning of the Christian era. In a final chapter the notion of 'land-hungry colonists' spreading into Europe is rejected in favour of the adoption of farming by the indigenous population; and its intermittent spread is emphasised.

The author writes with a nice awareness of the nature of early farming, a perspective often lacking from conventional textbooks on prehistory. In a book of this scope, it is perhaps inevitable that he has sometimes accepted other workers' conclusions, as well as their data, without critical discussion. However, the great strength of the book is that it contains more detailed information than either of the other two books on farming in prehistoric Europe which have appeared in the last few years: the third volume of the Research Project in the Early History of

Agriculture Early European Agriculture and Dennell's European Economic Prehistory. Though these latter sometimes contain greater insights into prehistoric subsistence, *Prehistoric Farming in Europe* is probably of greater value to archaeologists and students.

DALE SERJEANTSON

JAŹDŽEWSKI, Konrad. *Urgeschichte Mittel-europas.* Wrocław, Zakład Narodowy im. Ossolińskich, 1984. 528 pp., illus. No price set.

Professor Konrad Jaźdżewski died in April 1985. It is perhaps appropriate that, in the last of a long series of over three hundred publications, this most distinguished Polish scholar should present his very personal vision of Central European prehistory. With the ever-deepening, and seemingly inevitable, specialisation among archaeologists, the all-embracing work of this kind has become a rare species. When undertaken, such a work often reveals the author's shortcomings in fields outside his own speciality; but this is not the case with the present volume.

The Central Europe of Jaźdżewski's study is, as he himself observes, an arbitrarily delineated region of 1200 x 900 km, based largely upon modern political divisions but modified where necessary by geographical considerations. The work covers an immense timespan: from the appearance of the earliest hominids and/or the implements recognised as their products - sometime during the Günz-Mindel interstadial - to the formation of the medieval states (XII/XIII century AD) when the last vestiges of prehistoric communities, as exemplified by the Baltic Slavs, finally enter a fully historical period. Such an extended chronological framework may appear somewhat unorthodox to Western archaeologists, for whom prehistory generally ends with the expansion of the Roman empire. But it represents a commonly accepted chronology among Eastern scholars for whom the medieval (or at any rate early medieval) period forms a natural continuity in the economic, cultural and

socio-political developments in which archaeological and historical data tend to be used in supplement rather than in opposition.

Although throughout the discussion there is a certain encyclopedic quality to this book - at least in the sheer range of material covered - the reader never gets the impression that all the available data have been 'crammed in'. On the contrary, one is constantly aware of a conscious selection of material, carefully chosen to support a specific point of view or to pursue a particular theme.

The main part of the book follows a sound traditional chronological framework beginning with the Palaeolithic (chapter III) through to the Iron Age, the latter being extended, as already noted, to the XII/XIII century AD (chapter VII). Within each of the major chronological units we are first offered a general commentary based upon the relevant climatic, environmental and anthropological data. Then follows a discussion of the archaeological material which reveals a well-balanced descriptive and interpretative approach. Although naturally the emphasis upon particular themes varies from chapter to chapter, there is nevertheless a prevailing pattern to a discussion. Jaźdżewski covers themes such as settlement, economy, material equipment (ceramics, flint, stone or metal industries), burial and other rituals, social organisation and - in the later prehistoric period - political and ethnic issues. The latter in particular are treated with judicious authority - especially the vexed question of the relationship between the Germanic and Slavonic tribes.

While no-one would doubt Jaźdżewski's authority in matters of Central European archaeology, the quality of his scholarship is revealed even more clearly when he moves into fields obviously outside his immediate, already wide-ranging interests. Events on the periphery of Jaźdżewski's Central Europe are not discussed in detail but he fully appreciates that these played a considerable role in the shaping of the cultural, social, economic and, later, political developments in Central Europe. We are therefore offered, in frequent digressions, a broad background which extends to Africa and Asia in the early stages. It takes in the Near East, the Balkans and the North Pontic regions during the consolidation of farming economies, and it shows the impact of the Aegean

during the Bronze Age and reveals what in the Iron Age was absorbed from the Greeks, Celts, Scythians and Romans as well as from the Turco-Tartar nomads in the south-east or expansionist Scandinavians in the north. It is this broad vision of events, in which Jaźdżewski's discourse is as informed and eloquent as it is in matters closer to home, that makes *Urgeschichte Mitteleuropas* such a valuable book. It should become standard reading for anyone who wishes to gain an understanding of European prehistory.

The reader who wishes to use this book merely for quick reference is assisted by margin entries which identify in more detail individual themes discussed within each chapter. The book is also useful for its illustrations. Numerous maps, small but clear, illustrate the shifting centres of activity. The 197 illustrations are of high quality but tend to be biased towards the representation of artefacts. The inclusion of a few more site plans as well as more detailed chronological tables (note the use of radiocarbon and calibrated dates throughout!) might have improved this aspect of the book.

References in the text are few and far between and, when they do appear, it is mostly as numbers referring to positions in the bibliography. This is a serious shortcoming from the point of view of the reader who may wish to pursue a certain point or idea or trace some information to its original source. On the other hand it must be said how much more pleasant it is to read a text where the flow of words is not constantly interrupted by brackets and reference entries. The selected bibliography contains over 280 titles. General works are arranged by country, while more specific references are arranged chronologically following the order of the chapters. There are also separate sections covering such topics as linguistics, theory and methodology, geology, geography and anthropology. While an index is a regular feature of most British and West European publications, East European books frequently do not have this useful item. It is therefore pleasantly surprising to find that Jaźdżewski's book is accompanied by two indexes: one of persons' names, the other of geographical and cultural names.

Last, but not least, how many of us could write a book that will not only please from an archaeological point of view, but will also inspire linguistically? This reviewer has read two versions of this work -

the original, longer Polish edition as well as this shorter, German version written by Jaźdżewski himself. Both impress with their eloquence and their command of language. This work is a superb achievement by a pre-historian whose scholarly approach and personality have impressed themselves on European archaeology for over half a century.

MADGALENA S. MIDGLEY

TORRENCE, Robin. *Production and exchange of stone tools: prehistoric obsidian in the Aegean* (New studies in archaeology). Cambridge, Cambridge University Press, 1986. xiii + 256 pp., 56 figs., 35 tables. f27.50.

The title of Robin Torrence's masterly reworking of her doctoral thesis belies its breadth and significance. The study extends beyond the Aegean to the world in a conspectus of lithic raw material procurement and exchange in both prehistoric and ethnographic contexts.

A valuable review of recent analyses of exchange processes serves as a prelude to one of the cornerstones of the work. Proceeding from the premise that the more profit-oriented an exchange system is the more efficient production within it will be, Dr Torrence describes various means by which efficiency might be achieved in lithic procurement and reduction; defines their probable archaeological expression; and examines their incidence among modern and recent stone-quarrymen and workers. The level of correlation between some of the measures of efficiency and the social and commercial contexts in which the practitioners operate(d) is impressive.

The theoretical framework so established is used to examine the Neolithic and Bronze Age obsidian quarries of Sta Nychia and Demengaki on the island of Melos, and the distribution of their products. In addition to applying her criteria of efficiency, Dr Torrence assesses the annual person-hours

likely to have been expended at the quarries, and examines the occurrence of material from them in settlement contexts elsewhere in the region.

The results repay what must, despite a carefully designed sampling strategy, have been a vast input of her own labour. The density and nature of waste at the quarries seems, when viewed in the perspective of the many centuries during which they were worked, to represent an expenditure of no more than 133 person-days per year for both. Unsystematic extraction, lack of a specialised tool-kit, low levels of standardisation in working methods and in debitage, low estimated annual production of macrocores (the main product), and evidence for the export of unmodified nodules all point to unorganised, non-commercial exploitation. Direct access by obsidian-users to the sources is inferred. The conclusion is supported by examination of industries from settlement sites elsewhere in the Aegean, which show little fall-off in flake size with distance from the quarries, suggesting that much of the material was collected at little marginal cost, perhaps in the course of fishing expeditions.

These conclusions weigh heavily against traditional views of the exploitation of Melian obsidian as a major commercial undertaking, which contributed to the development and prosperity of the city of Phylakopi.

A possible misgiving is that the ethnographic record, one of the foundations of Dr Torrence's approach, is irremediably flawed and fragmentary. Adequate anthropological and technological accounts exist for only a handful of stone-working societies out of what was once a whole world-full. If more of them were better documented, would the present correlations between society, economy and technology still obtain?

Whether they would or not, Dr Torrence has driven home the practical point, all too often forgotten, that the superficially vast bulk of debitage which characterises mine, quarry and workshop sites can actually be generated at relatively little cost in time and labour. The temptation to infer commerce, even industry, from bulk alone has to be resisted. How the material was worked is at least as important as how much of it was worked. On the general level Dr Torrence has provided a model of the construction of theory appropriate to the data and of its

realistic application, and has demonstrated the need to view lithic procurement and distribution in their complete human context rather than as a specialised aside to settlement archaeology. She has made us put down the calipers and think.

FRANCES HEALY

PREZIOSI, Donald. *Minoan Architectural Design: Formation and Signification* (Approaches to Semiotics 63). Berlin, New York, Amsterdam, Mouton Publishers, 1983. xxxi + 522 pp., numerous plans and tables. £39.95.

This maddening but important book stems from a Harvard Thesis of 1968 on Minoan Palace Planning and its Origins. It is maddening for the jargon and convoluted language which make reading and appreciation difficult: 'synecdochal' (of revelations), 'contrastive' and 'connectivity', are among words which besplat the opening pages. But 'megastructure', although a monstrous hybrid, is admittedly convenient as a non-emotive term for a palace. Arcane and sometimes arrogant pedantry masks profound thought and deep understanding of the principles of Minoan architecture, however, appreciated here as never before. Those who struggle through the maquis of jargon to the account of the Minoan Hall system and the detailed analyses of houses and palaces will be well rewarded.

Preziosi shows how Minoan buildings were planned, and how, while each was different, and symmetry as found in temples and palaces of contemporary Egypt was avoided, the arrangement of their parts followed definite principles even if in a highly flexible manner. Part One: Formal Organization is concerned with this. An Appendix (A) contrasts the underlying principles of Mycenaean architecture, exemplified in Crete by Gournia House H-e and by the complex excavated by the British School in 1913 at Plati in Lasithi. The approach seems right in general; but House H-e at Gournia is not

yet fully excavated, and the remains at Plati, interpreted by Preziosi as those of a LM III palace, look from the published photographs (with their abundant use of dressed stone) to be LM I rather than LM III in date and to belong to several independent buildings: the British excavators assigned them to LM III, but in a very offhand manner, and did not illustrate or even describe a single LM III vase or sherd.

Part II on Modular Organization deals with the way in which Minoan buildings were planned and laid out on the ground before construction. Preziosi makes generous acknowledgement of indebtedness to A. Badawy's parallel work on Egyptian architecture. The basic Minoan unit of measurement seems to have been a 'foot' of c.27 cm; another of c.34 cm in less common use appears to correspond to a widespread family of units of measurement ranging from ancient Sumeria to the Indus Valley in the east and Northern Europe in the west. Both long and short 'foot' were used together in the palace at Knossos and in the later one at Phaistos. Preziosi also claims that some of the early circular tombs of Crete were laid out with the long and others with the short foot. Preziosi emphasises the important role played by religion and ritual in the laying out of the palaces. He cites the work of Scully with approval, and suggests that sunrise on a special day may have been the decisive factor in determining orientation.

The system for laying out a Minoan building involved an original square or rectangle established with the help of diagonals and divided into (or in the case of some larger buildings supplemented with) a grid of equal squares. These were established on the ground with pegs and rope: each had sides consisting of a number of 'foot' units - usually 10, 8, 6 or 5. Preziosi's reconstructions of how specific buildings were laid out are often most convincing. In the palace at Knossos, for instance, his scheme for the West Magazines based upon a 'foot' of c.34 cm is attractive; but the initial square formed from a pair of equal rectangles, one covering the Central Court, the other the west wing apart from the Magazines, ignores the line of the original west facade of the Court, and in general Preziosi avoids the problem of different periods of construction. His idea, several times repeated, that the Vat Room Deposit was a central foundation deposit for the Early Palace is hard to accept, as is the suggestion that double axe signs (masons'

marks) were cut to mark nodes in the original grid layout for the palace: double axes being the commonest sign at Knossos, scattered throughout the palace and occurring in houses and tombs, and dating from many different periods. Preziosi's remarks on the orientation of the palace ignore the fact that the west facade adopts the line of a previous megastructure (to use his term) dating back to EM III. In discussing entrances to the palace Preziosi omits the admittedly hypothetical North-East Postern of Evans.

Some curious ideas might have been eliminated by discussion or closer inspection. One is that the Koulouras in the West Court at Knossos and elsewhere were built as pits for planting trees. The raised square altars in the West Court at Knossos are dismissed as paving; but that originally proclaimed by Evans in the 'Court of the Altar' in the palace is accepted, although it was later recognised as a patch of paving as can be verified on the site today. At the same time original and interesting ideas abound: such as that House B at Tylissos was an annexe joined by a bridge to House A, like the Unexplored Mansion to the Little Palace at Knossos, and perhaps (Preziosi suggests) the Northwest Treasury to the palace there. After Preziosi's demonstration of their key position in the layout of major buildings and palaces it will be difficult to argue that Pillar Crypts were not used for ritual. An intriguing suggestion is that Minoan craftsmen may have worked on the pyramid of Senusret II, leaving a memorial of themselves at Lahun in the form of two unique and un-Egyptian wooden measuring rods.

Preziosi may be right to reject J. W. Graham's Minoan foot of 30.36 cm akin to a foot current in later Greece. He accepts Graham's identification of dining halls at Phaistos and Zakro, but dismisses that over the pillared hall (Teloneion) beyond the North Entrance of the palace at Knossos in favour of one above the Corridor of the Bays / Magazine of the Medallion Pithoi area, although north of the Central Court seems the place for such halls in other palaces. Preziosi appears rather grudging in acknowledgement of the pioneer work of Graham on Minoan architecture, and there is no mention in text or bibliography of J. W. Shaw's classic *Minoan Architecture: Materials and Techniques*, which was published ten years earlier in 1973, nor of G. Cadogan's *Palaces of Minoan Crete* (1976). Misprints are

mostly quite minor, but the figure and title of the plan at Gournia are absent on p.255. The attractive and helpful isometric drawings would have benefited from north signs, which are missing from some of the plans as well.

No criticism, however, should obscure the fact that this is an important and original book which adds a great deal to our understanding of Minoan architecture and of life in Bronze Age Crete in general.

SINCLAIR HOOD

ANDERSON, Anne C. *A Guide to Roman Fine Wares* (Vorda Research Series 1). Highworth, Vorda Publications, 1980. 58 pp., 15 figs. £3.25.

The greatest flaw in this book is its misleading title. Rather than being a general guide to Roman Fine Wares, it is specifically a modest introduction to the fine drinking vessels in colour-coated fabrics produced by the better known industries of the Rhineland, Gaul and Britain. The date range is similarly limited to AD 70-200. The first date was chosen to follow on from Kevin Greene's work at Usk, the second, with less justification, to coincide with the cessation of major importation of samian into Britain.

Within the above restrictions the book gives a handy summary of colour coated beaker forms, each drawn in half scale. The text is pleasantly uncluttered by references, although this clean presentation means that most statements regarding dating, development and distribution have to be accepted at face value, which limits the book's scholastic potential. Although the author did not intend to include analytical information, it must be felt that the 'fabric' definitions are inadequate for field identification, being largely qualitative descriptions of colour and texture. Describing a fabric as being 'rather sandy' is too imprecise to be useful. Already showing its age, the book was written before current controversies arose concerning the correct identification of British colour-

coated beakers, 'Rhenish Ware' and 'North Gaulish Ware'.

JASON MONAGHAN

RENFREW, Colin. *The archaeology of cult: the sanctuary at Phylakopi* (Supplementary volume no. 18). London, published for the British School of Archaeology at Athens by Thames and Hudson, 1985. xiii + 513 pp., illus., 6 pull-out figs., 2 figs. in pocket, 70 pls. £35.00.

The site of Phylakopi on the Cycladic island of Melos was first excavated on behalf of the British School at Athens at the end of the last century and briefly in 1911. In 1974 the site was investigated again under the direction of Professor Colin Renfrew in order to establish a detailed stratigraphic record particularly for the less well understood LBA periods. The discovery of the important sanctuary area and associated finds ensured that work continued at Phylakopi for another three seasons until 1977.

In this, the first of two volumes of reports, the focus is on the two shrines in the sanctuary area, while the second intends taking a wider view of the settlement from the beginning of the Bronze Age. The sanctuary with the famous figure of the 'Lady of Phylakopi' had already before the publication of this comprehensive report achieved a high profile in Bronze Age Aegean studies through a series of publications by Renfrew over the last 9 years or more and assumed a significance not far behind that of the contemporary 'temple' and cult centre at Mycenae.

Chapters 2 and 3 provide exhaustive and clear accounts of the excavation establishing the history of the two shrines in the sanctuary from the construction of the earlier West Shrine in LHIIIA2 to that of the East Shrine in IIIB1 and the abandonment of the area late in IIIC. The chronology, whose micro fine divisions are impressive in a prehistoric context, is based on synchronisms with the pottery of the Argolid and suggests

some 270 years of use for the sanctuary from 1360 to 1090 BC. Chapter 4 discusses the sanctuary stratigraphy in detail and the finds. The sanctuary sequence over three main phases, including a destruction phase in 2b, is complex. But a synopsis on p.7, numerous tables and plans, particularly 4.1 at the back of the book, and the stratigraphic diagrams in Appendix A help considerably. The reports in chapters 2 and 3, when read together with the discussion in Chapter 9, pp.377-84, give a clearer picture of the diachronic history of the area than autopsy ever could. Useful diagrams of the assemblages (except for E and F) show the sequence and location of the finds in and about the shrines until their abandonment in phase 3c when the sanctuary was filled with stones. One consequence of this detailed procedure is that important finds are discussed more than once under various headings. This aptly reinforces the reader's mental picture in the various special reports but can be repetitive in the exegetic chapters of the book.

In Chapter 5 P. A. Mountjoy deals with the pottery and Elizabeth French with the terracotta figures (15 cm and bigger) and figurines from the sanctuary in 6. Her discussion of this Cycladic material in the context of mainland and Cretan traditions of votives is particularly valuable for an understanding of the intricate interrelationship of all three cultures. After an assessment of seal stones in Chapter 7 by John Younger, Renfrew and John Cherry examine the other finds from a scarab (note by Dr T.G.H. James) to the gold 'mask' from the East Shrine, the 'Smiting' figures, a bronze bird and all other objects. The scattered fragments of ostrich shells in the East Shrine are an interesting indication of the nature of the cult there. (The incidence of such gifts including the use of ostrich eggs as cult vessels in the Mediterranean has been collectively discussed most recently by A. Finet, 'L'oeuf d'autruche', *Stud. Paulo Naster II, Orientalia antiqua*, Louvain, 1982, 68-77, and A. Caubet, 'Les oeufs d'autruche au Proche Orient ancien', *RDAC*, 1983, 193-8. R. Laffineur proposes an interesting explanation of their cultic significance, 'Fécundité et Pratiques Funéraires en Egée à l'Age du Bronze', *1st Int. Conference on Archaeology in the Mediterranean, Malta 1985*, U.P. Malta, 1986, 79-96). In the last two chapters Renfrew presents an overall view of the religious sanctuary in the Melian and then in the wider context of Aegean cult practice as a whole. Some five technical appendices,

bibliography, index and 70 plates complete the volume.

What traces of cult survive from Phylakopi in LBAI are Minoan. (Renfrew does not count the pillar room in the III City (pp.345, 375, 393). This is Rutkowski's view which is no longer universally accepted.) In the sanctuary area there is no firm evidence of earlier cult, although some figurines from there could antedate the shrines (p.262). By the time of the construction of the earlier West Shrine in LHIIIA2 (1360 BC) the bond was more direct with the Mycenaean mainland whose influence extended over the other islands as well. The most impressive figure of the 'Lady of Phylakopi' had in fact been imported from the Argolid. It is worth remembering, however, that the decorations on the figure were Cretan inspired (p.215), showing the cultural *koine* in Crete and Greece at the time (LHIIIA1) (p.279). It is unlikely that the cult at Phylakopi would have seemed alien either in Knossos or Mycenae. Neither was it necessarily identical, judging from the special architectural features of the shrines and the finds in them.

The two rooms west of the West Shrine are remarkable: niches communicating with the 'cella' and displaying votive figures, which were visible from the main shrine, suggest a kind of inner sanctum or *adyton* and recall a similar feature in the 'temple' of the Mycenae cult centre. A later extension wall (phase 1c) sealed the southern entrance to the shrine without, however, affecting the worship. After a collapse in 2b, a blocking wall inside the shrine cut off Room A and the s/w platform with the niche. Cult nevertheless still continued unabated and figures were re-used in the subsequent 3a phase. The 'Lady' had been put away by now in Room A with her companion (Assemblage C; for Room B read A on p.112), but smaller figures took her place in an impoverished but continuing cult until the end. Also notable is that the construction of the East Shrine in IIIB, together with an extension wall and city wall eastward from the east of the West Shrine, created a kind of court from the earlier open space. Both doors of the East and West Shrines looked onto this court which was equipped with a bench, a quarter circle of stones possibly for libations, and a shaped stone baetyl.

The unusual male figures from the West Shrine have few contemporary Aegean parallels apart from Tiryns. The three were grouped

about the new n/w platform (Assemblage G) and separate from the votives (including the Late Psi type, Assemblage B) in the s/w which after the collapse were moved near a new platform on Floor 2 in the n/e of the shrine. Renfrew suggests a division in worship between a female and a male divinity, and indeed the 'Lady' in Room A and her female companion were also connected with the s/w part of the shrine (p.373). The n/w platform continued with its niche into the post-collapse phase 3, although one 'female' figurine now found its way into the 'male' club (Assembl. G and K). In the East Shrine there were no human figures before 3c. The important gold 'mask' from there (3c) may have covered the wooden core of a cult figure (p.302).

Two bronze figures (or figurines by Dr E. French's definition; Renfrew uses both terms on pp.303-6) of the oriental 'Smiting God' are also of considerable interest. Both were imported from Syria in the 13th century (LH IIIB/C) and, although neither was found in a shrine, link Phylakopi to the eastern tradition of the 'Warrior God' type which modelled for some of the earliest Greek sculpture. Relatively few contemporary parallel figures have turned up in the Aegean (eg. Tiryns and Mycenae) and all clearly of the eastern type. Renfrew underestimates the importance of Cyprus (p.310; cf. 436) as an intermediary between east and west and active developer of the type through to the archaic and classical age especially in the Apollo figure at eg. Limniti and Tamassos (Frangissa and Pediaeus sanctuary).

Other parallelisms with Cypriot practice suggest themselves in addition to the figure of the 'Smiling God'. Dr E. French remarks on the comparability of the terracottas from the Phylakopi shrines with contemporary Cypriot dedications (p.277). Further the arrangement of cult rooms or shrines about a court, which in the case of Ayia Irini also had a central baetyl, is familiar from Cyprus, as well as the siting of the sanctuary area near the city wall. Evidence from both shrines at Phylakopi of tortoise shell instruments (p.325), which may have been used in cult, also recall a similar use of music and dance in Cyprus' rustic sanctuaries like Kourion where prehistoric traditions survived into archaic times. Finally one of the male figures from the West Shrine with breasts and male genitals (SF1553) has a few earlier parallels like the Kamilari group in Crete but in pose and form closely resembles the bisexual Cypriot centaur (p.223).

The discoveries at Phylakopi are exciting in their own right and important in a wider Aegean context at the end of the Bronze Age. In relating the finds from the sanctuary to contemporary cult activity in the Cyclades (Ayia Irini on Keos), Greece and on Crete this book exceeds the usual limits of an archaeological report. The title, *The Archaeology of Cult*, promises a more fundamental discussion of methodology in the study of prehistoric religion. This the author provides with great confidence, and in the preface and first chapter he devises a theoretical framework for the analysis of purely archaeological data. Rigorous and complicated sets of criteria are established for telling sacred finds from secular through an assessment of the buildings on a site, quality and scale of finds, possible presence of religious symbols, icons, etc. in the local and also wider context of other known sanctuaries. The method refines and therefore improves on more orthodox criteria, but is itself partly dependent on unverifiable assumptions and therefore also fallible.

There is some criticism of historians of religion for discovering sanctuaries or cult places too readily without sufficient critical control, for projecting back from known historical practice to the Bronze Age, or for taking an overall synchronic view of Minoan and Mycenaean cults. Much of it again is justified, especially concerning the analysis and interpretation of the growing mass of monumental remains. But there are obvious limitations in this kind of evidence, and it is rarely convincing to equate changes in artistic styles with social or religious upheavals. Sometimes Renfrew falls under the spell of his own methods in applying them to wider concepts like religion which defies easy definition, certainly in one or two pages (11-12). It also seems unproductive to ask (p.394) 'Was there a Minoan/Mycenaean religion?'. Such convenient modern terms of reference possess no intrinsic semantic significance on their own. It would be equally unhelpful to put the same question about Greek religion as if it had been a homogeneous science.

In the perennial debate about distinguishing between cult images and other kinds of votive dedications size is perceived to be a deciding factor. Thus the largest of the male figures in the West Shrine (SF1550) is described as a cult figure for that reason, and because it carried nothing in its hands (225; 372). That seems right.

Bits and pieces were found in different assemblages (A and G) showing that the figure was re-used in cult in a later phase. But others, though smaller, may also have had more than votive status in the shrine, notably the most elaborately decorated figurine of both assemblages (SF 1553) whose gesture is similar to that of 1550 (although it may have been holding an object in its left hand) and which is clearly bisexual. In another place in fact Renfrew argues (p.417; cf. p.439) that small figurines in the West Shrine stood proxy for the single large cult image ('Lady'). Size could not have been the only or even decisive criterion for a cult statue. For instance, the divine credentials of the monumental figures in Ayia Irini on Keos have not been established beyond reasonable doubt (cf. p.432). The error potential increases geometrically when Renfrew extends the argument outside Phylakopi to the large Early Cycladic marble figures which he now claims as ancestors of the Melian males over a millennium later (p.435; p.438).

The religious historian will be more interested in the bisexual nature of SF1553, curious ambiguity of sex in some of the other figures, and indeed apparent sexlessness of the majority of the dedications. The other Phylakopi males leave no doubt as to their sex, but the same cannot be said of practically all of the 'female' figures including the 'Lady of Phylakopi' whose beard casts doubt on her/his own gender. The explicitly female companion (SF2658; for Plate 56a on p.415 read 34 d, e) might well have been intended to express polarity of sex or at least of function between the two figurines. Dr French notes the lack of sexual accentuation of most mainland figures (p.216) but assumes female gender for all without clear male attributes including the examples from Phylakopi (p.211). Renfrew follows her and 'most writers' but adds his own qualification of an 'indication of breasts, and the general impression of grace' (p.415). From a cultic point of view, however, it might be better to admit sexlessness and bisexuality in contemporary religious belief as indeed it appeared in other Aegean contexts from the Stone Age.

Renfrew links the Phylakopi males with the appearance of male terracottas in the open air sanctuary (Piazzale dei Sacelli) in Hagia Triada in LM IIIC and also with the well known Cretan male bronze figures mostly from caves, but generally dated later to

Subminoan or Protogeometric times. Both Cretan types he believes to have descended from the Melian males which could have been made in LH IIIB rather than in the succeeding period. The Cretan bronzes clearly continued earlier traditions and Renfrew redates them to the end of the Minoan period in order to establish an unbroken line to the end of the Bronze Age. The males, according to Renfrew, are figures of Cycladic western gods unlike the oriental 'Smiling God'. He identifies them in fact as the icons of the long lost gods whose names have been read on the Linear B tablets and who appear in the classical Greek pantheon of Olympians (pp. 420; 423-4; 440).

If correct this would underpin Renfrew's main thesis that there was no break in religious tradition during the Dark Age but earlier in the LBA when the palaces came to an end in Crete and on the mainland (Knossos IIIA, Mycenae LH IIIB). Renfrew speaks of a system collapse ending centralised 'state' religion and leading to the spread of a 'popular' or 'folk' religion which was celebrated in new 'domestic' shrines. Archaeologically this social upheaval was signalled by the appearance, in Knossos, Gournia, Kania, Prinias, Gazi, Karphi, etc., of a novel kind of cult assemblage principally consisting of the well-known Goddess with Upraised Arms (GUA) in her own separately built 'domestic' shrine. Renfrew links this phenomenon with the construction of the West Shrine in Phylakopi and the figure of the 'Lady' (whose arms unfortunately are lost) in Room A. Phylakopi's closest bond at the time was with Mycenaean culture, and the goddess was indeed Mycenaean, according to Renfrew, as was her characteristic gesture (a 'general attribute of holiness and sanctity', p.432). But ultimately she had been Minoan-inspired, showing a 'reflux' at the time of Minoan religious forms from the mainland and a cultural *koine* in the western Aegean (p.437).

Renfrew's conclusions perhaps over-stretch the Melian data in order to integrate the sanctuary in the sequence of events in Crete and Greece. His case would also be stronger with a wider range of comparative controls than the three contemporary Minoan shrines (Knossos, Gournia, Gazi) and the scenes from the Hagia Triada sarcophagus. (Gesell's excellent study provides a diachronic survey, *SIM* LXVII, 1985. Renfrew does not mention her in his bibliography nor any work of Schachermeyr for that matter.)

However, Renfrew's conclusions are important and not implausible in themselves but the evidence is not always unambiguous. For example, in Mycenae palace and cult centre ended together in LH IIIB. But at Tiryns, which had some of the closest parallels with Phylakopi, shrines and effigies in the 'Unterburg' continued beyond the palace thereby clouding the impression of a break.

A more fundamental question concerns the extent to which information about social or cultural customs can convincingly be extracted from the purely material remains. Bench shrines were not new in post-palatial times. Separate natural and constructed sanctuaries, too, had a long history: apart from caves, they did of course exist on peaks and elsewhere both within and outside settlements. Rural sanctuaries have disappeared but are shown on rings and seals. The GUA herself, together with bull and her other animals, had antecedents in Crete. She was never stereotyped but varied considerably in form, even the position of her hands and arms was rarely the same from one figure to another. Her facial expression, too, shows a surprising variety, and it is anyone's guess if and when such differences reflected changes in artistic style or in her nature and functions. Her various attributes on individual icons do occasionally suggest the latter, but the distinctions are far from clear, and at the end of the book Renfrew more plausibly speaks of her image as representing '*a class of divine personage*', perhaps the Potnia of the Linear B tablets (p.433).

The absence from Phylakopi in its Minoan phase of the two best known religious symbols, the horns of consecration and the double axe, is remarkable and appears to point to differences in cult (pp.389; 395; 429). Renfrew discounts the imported ivory ring as irrelevant. The horns of consecration, which are shown on it, constituted an alien symbol for the Phylakopians in the same way as the cultic scenes on the Minoan type gold rings from the Shaft Graves at Mycenae and elsewhere on the mainland were incomprehensible to the natives. According to Renfrew, who follows Vermeule's explanation, Minoan princesses brought the rings with them as a kind of dowry and the Mycenaeans recognised the Cretan religious symbols on them but not their true meaning (p.399). The point is arguable but leads to Renfrew's most interesting example of what Glyn Daniel called controlled imagination in the interpretation of

the archaeological data.

Religious symbols, he maintains, travelled beyond their home but without their original significance. Where the same symbols occur in different places in a region, they signal a community of religious expression but not identity of cult and cult figures. Such symbol systems resulted from a kind of 'peer polity interaction' which means, I think, that neighbouring states borrowed the outward forms of cult from one another but not its substance. The theory applies equally to whole common cult assemblages like the GUA in her shrine with animals, votives, etc. and to individual items like double axe and horns of consecration which presumably in their new context became decorative objects rather than sacred symbols and carriers of divine power (eg. pp.367-8; 394-5; 396).

The Archaeology of Cult abounds with such ideas and suggestions of new methodology which are often persuasive and even when they are not are always deserving of serious consideration. The book is a model of collective expertise and definitive on the fascinating sanctuary at Phylakopi. On the wider issues of contemporary Aegean religion Renfrew's contribution is enormously vital and provocative but inevitably his methods remain theoretical and their limitations have to be recognised.

BERNARD DIETRICH

FRAYN, J. M. *Sheep-rearing and the wool trade in Italy during the Roman period* (ARCA, 15). Liverpool, Francis Cairns, 1984. 208 pp., 13 figs., 8 pls.
£20.00.

It should be said at the outset that this is a useful and well written book. It has something of value for those interested in Roman studies and for environmental archaeologists, although it appears to be aimed primarily at the former group. Its ten chapters deal with every aspect of sheep-rearing and wool production, beginning with

a consideration of the geographical distribution of sheep (and the suitability of different zones for sheep rearing), and continuing, in Chapter 2, with a description of Roman sheep breeds. The next three chapters consider the husbandry and way of life of Roman shepherds, showing that transhumance was well organised and recognised by the state, but that shepherds were seen as something of a social anomaly. Chapter 6 is a brief survey of the writings of Roman agonomists (who are, however, referred to throughout the book). Chapter 7 describes Roman ranching, and shows this to have been separate from transhumance, with sheep herds of considerable size being owned by wealthy landowners whose 'ranches' were very extensive. This appears to have been a fairly late development, and was sometimes seen as an unwelcome change, for example in a passage from Seneca, quoted at length on p.111. Chapter 8 discusses dairy produce, showing that the production of cheese was, perhaps, the most important aspect, and Chapters 9 and 10 consider the production, processing and trade of wool.

There are, however, a few criticisms of this book, mainly in relation to the style of publication. It is a slim volume (208 pages), which at £20 is very expensive, yet there are few illustrations (13 figures and 8 plates). In some cases the illustration quality is poor (Figs. 1 and 3), and the copy supplied for review had a major printing fault on p.28: a blanked-out area. Poor editing is evidenced by errors such as the repeat line printed on p.23 and miss-spelling of withies on p.131. The notes to the text are printed at the end of each chapter, making them difficult to consult. They would have been better printed as true footnotes, or in one section at the end of the book. The figures are only seldom referred to directly in the text, most references occurring in the chapter notes, and some not referred to at all. Finally, although the latin is italicised throughout, the single scientific name used (that of lungwort, *Pulmonaria officinalis*) is not (p.104). These are all relatively minor points, but they add up to indicate a fairly poor level of editing and style of publication.

The main asset of this book is the extensive use of contemporary Roman writers, who are well quoted and referenced. The literary, rather than scientific, training of the author is evidenced by the minimal use of archaeological, and especially envi-

ronmental, evidence. The discussion of sheep breeds, for example, would have been much enhanced by a consideration of the evidence from bones and woollen remains. Generally, however, this is a useful addition to the study of sheep husbandry, and should be recommended reading for any student or researcher of the subject.

BRUCE LEVITAN

BLAZQUEZ MARTINEZ, J. M. and REMESAL RODRIGUEZ, J. (eds.). *Produccion y comercio del aceite en la Antiguedad: Segundo Congreso Internacional*. Madrid, Universidad Complutense, 1983. 616 pp., 250 + figs., plates and tables. Price not stated.

REMESAL RODRIGUEZ, J. *La 'annonia militaris' y la exportacion de aceite Betico a Germania: con un corpus de sellos en anforas Dressel 20 hallados en Nimegan, Colonia, Mainz, Saalburg Zugmantel y Nida*. Madrid, Universidad Complutense, 1986. 283 pp., 23 figs., 18 tables. Price not stated.

The two books reviewed here present a wealth of new information for historians and archaeologists interested in the economy of the ancient world. Olive oil was a vital component of that economy, though the information relating to its production and trade has rarely received the attention which it merits.

The published proceedings of the second conference on *Produccion y comercio del aceite en la Antiguedad* (the first conference was published with the same title in 1980, also by the Universidad Complutense) comprise 24 of the 25 papers given at the conference. In a short review it is not possible to mention each individual paper and I shall concentrate on wider questions. Firstly, it must be stressed that the volume would be more appropriately titled 'Comercio y Produccion', since the vast majority of the papers deals with evidence for trade, with a

particular bias towards amphorae studies - as is made clear in Blasquez Martinez's lengthy, but very useful, survey of recent developments. This is one out of a total of 15 papers relating to typologies, distributions and above all to stamps on amphorae, most of the discussions centering on the globular Dressel 20 form from southern Spain. Only a few papers deal in any detail with the evidence for massive exports of olive oil from North Africa (Beltran Lloris, Keay, Panella), but D. Manacorda's demonstration of the potential for prosopographical study of stamps on Tripolitanian amphorae can only serve to increase interest in the rivals to the Spanish industry.

Whilst the geographical spread of research is impressive (covering sites in Britain, Germany, France, Spain, Italy, Switzerland, Yugoslavia and the Eastern Mediterranean), there is still an urgent need for greater attention to be paid to the far larger assemblages of the towns of the Mediterranean littoral. Greater standardisation is required, however, in the way in which amphorae are recorded, preferably adapting the weighing and sherd counting methodologies advocated by Peacock and Williams and Panella, which allow for much more subtle, quantitative, analysis of the data.

Several papers are concerned with broader issues concerning the technicalities and organisation of the Spanish oil exports (notably Ponsich on the topography of the River Guadalquivir and Rodriguez Almeida on epigraphic evidence from Rome). Remesal Rodriguez, in reporting a new excavation of a kiln site on the Guadalquivir, described important evidence for changes in the production of Baetican amphorae during the third century AD.

Under 50 pages are devoted to evidence for olive presses, though this includes a useful overview of the Spanish evidence by M. C. Fernandez Castro (pp.568-99). One hopes that this imbalance can be corrected at future congresses.

The editing of the volume has been very much *laissez-faire*, with no evident attempt to standardise the presentation of the papers. The absence of a full list of figures, plates and tables is to be regretted, though the editors' priority was clearly to achieve rapid publication of the proceedings. There is no doubt that they have produced a substantial and very useful volume.

Remesal Rodriguez's book on the '*annonaria militaris*' is also an important work. The corpus of known Dressel 20 stamps from six sites on the German *limes* is valuable in itself, but the commentary and analysis provided by the author vastly increase its significance. The first section concerns the history and methodologies of amphora research, whilst the second explains the basis for dating the Dressel 20s by their shape and rim forms or by their stamps. The third chapter (pp.35-70) is perhaps the most thought-provoking, with statistical information derived from the corpus presented graphically. Remesal Rodriguez's knowledge of the kiln sites in Baetica is much apparent here, with 445 of the 633 stamps being assignable to specific production sites. About 40 kiln sites are represented, but of the 445 stamps, 97 (22%) came from a single site - La Catria - with a further 73 (16%) traceable to sites in close proximity to La Catria. Another major zone of production centred on the small towns of Arva and Canama downstream from La Catria (with 24%). Two other sites, Malpica and Las Delicias, made up the bulk of the rest (19%). Over 80% of the stamps, therefore, came from 5 major sites or other kilns in close proximity to them. Remesal Rodriguez analyses the implications of this, in a series of graphs which compare the relative numbers of stamps from the five main production centres at each of the six German sites and which also illustrate the changing pattern of supply through time. The comparatively large numbers of stamps from each of the six German sites (varying between 85-137) suggest that there is a reasonable chance that the trends detected are significant.

In the final chapters (pp.81-112) Remesal Rodriguez considers the evidence for the organisation of the *annonaria militaris* and what the implications of this might be for our interpretation of the transport of bulk supplies of olive oil to the German *limes*. Whilst his conclusions remain speculative in some key areas, there is no doubt that this discussion represents a useful marshalling of the available evidence.

The corpus of stamps (accompanied by 1:1 drawings) will be a major source of reference for archaeologists, particularly as it is clearly organised. The main listing is in alphabetical order by *nomen* or what is assumed to be the *nomen*, following Dressel's original practice (eg. L.A.F. is listed under A, next to G.A.F.). In cases where a

single name only appears on the stamp, generally a *cognomen*, they are listed at the end of the letter section (eg. SATURNINI follows the *tria nomina* entries for S). Subsequent indexes list all stamps alphabetically by both first and last letter to assist location of particular stamps within the corpus. Another index gives an abbreviated list of all the stamps with indications of which of the German sites they were present on, their date and production site where known. There is also a full table of concordance with CIL XV and with Callender's catalogue of stamps.

D. J. MATTINGLY

RUSSELL, H. F. *Pre-Classical Pottery of Eastern Anatolia* (BAR Int. Ser. 85). Oxford, British Archaeological Reports, 1980. vi + 176 pp., 31 figs., 6 tables. £8.50.

The author has performed a most useful service to those interested in early Anatolian ceramics by analysing and classifying a hitherto unpublished corpus of prehistoric and pre-Classical sherds collected in 1956 by Charles Burney. Although these were surface finds, they were recovered from eastern Anatolia, that vast, mountainous area, not well-known archaeologically, which lies between Malatya and Lake Van. He has also included some sherds collected from a more southerly area (Adiyaman Province), surveyed by himself. The periods dealt with are Neolithic, Chalcolithic to 1st Millennium (sherds of later periods having already been published by Burney).

Before attempting to classify Burney's sherds, the author carried out extensive comparative studies, not only visiting the excavations which had taken place since Burney's time (several of these being tells visited earlier by Burney), and discussing the material with the excavators, but also examining sherd collections of early pottery in museums. Thus he was made aware of the relative stratigraphic - and often chronological - position

of many sherd types present in the surface material. He has wisely taken into account the classification methods used by those dealing with excavated sites, eg. M. K. Buccellati at Korucutepe and A. Palmieri at Arslan Tepe among others.

The sites from which the surface collections were made fall naturally into the following groups, separated from each other by topographic barriers or unsurveyed terrain: Van, Muş, Elazig, Malatya, Adiyaman. These sherd groupings are interpreted based on further grouping into 39 ceramic types under their conventional names, such as Dark Faced Burnished Ware, Halaf Ware or Transcaucasian Ware, etc. Their distribution and incidence is presented in tables, and the sherds are well illustrated. There are catalogues of sherds, of sites, and maps of each region are provided; the bibliography is comprehensive up to 1980.

The author discusses the many archaeological problems connected with Eastern Anatolian ceramics in the light of available C14 dates and other recent studies; one example is the question of the applicability of Braidwood's term 'Dark Faced Burnished Ware' to any but Amuq Plain sherds and another is the controversy over what is 'true' and what is 'local' Halaf. Although many such problems remain unresolved today, the author has added to the body of knowledge available from a remote region which badly needs further research.

L. COPELAND

CANBY, Jeanny Vorys, PORADA, Edith, RIDGWAY, Brunhilde Sismondo and STECH, Tamara (eds.). *Ancient Anatolia: aspects of change and cultural development. Essays in honor of Machteld J. Mellink* (Wisconsin Studies in Classics). Madison, University of Wisconsin Press, 1986. 120 pp., illus. \$35.00.

Compared with the monumental *Festschrift für K. Bittel: Beiträge zur Altertumskunde*

Kleinasiens, Mainz, 1983, this Festschrift for Professor Mellink is a modest offering of eleven, mostly short, essays. With her wide range of interests which embraces Bronze and Iron Age Anatolia and its contacts with the Aegean and its chronology, archaeology, foreign relations, art and religion, one might perhaps have expected the editors to have widened the choice of contributors a little more. There are numerous others who would willingly have contributed an essay in her honour.

Every reviewer has of course his own preferences, based on his interests and as reviews, in my opinion, should be constructive (why read waspish ones?) I shall concentrate on T. Özgüç's essay (p.31-47), on the relationship of Kültepe with southeast Anatolia and North Syria in the third millennium BC. The evidence here presented is vital to Anatolian EBA chronology and trade relations, as it deals with imports into Kültepe from at least three different regions: North Syria, west of the Euphrates and Cilicia; North Syria in the Euphrates, Balikh and Khabur areas, and in rather vague terms West Anatolia. One might note the absence of imports from the East Anatolian (Transcaucasian EB3 province) or of anything that is confined to Assyria or Southern Mesopotamia, barring lapis lazuli beads that must have passed through these territories from its source in Badakhshan in Eastern Afghanistan.

Although C-14 dates appear to be regrettably absent (in 1957 when these excavations were conducted its significance had not yet sunk in!), T. Özgüç now has a fourfold division of the EB3 period at Kültepe (from top to bottom, levels 11a, 11b [burnt], 12 [burnt] and 13 [burnt]) between level 10=Karum IV=MBI and levels 14-17, which are EB2.

On the basis of southern imports the Kültepe sequence can now be linked to the Mesopotamian scheme. The evidence available at present suggests that trade relations can be demonstrated between Kültepe and its neighbourhood (Frakkin) and the Euphrates at Malatya as early as final Ubaid, c.4500-4000 BC. Incidentally the earliest culture within the Halys basin, the Late Chalcolithic of Ikiztepe on the Black Sea is of the same date. Kültepe is about half way between it and the Euphrates and at the dawn of Anatolian history (Kültepe Ib) a king of Mama, Anumhirbi, whose territory is adjacent to that of Kanesh, campaigned over this same long stretch of territory, setting up a victory monument on Mount Adalur (Kurt

Dag) between Aleppo and Maras in the south and battling with the king of Zalpa (Ikiztepe) in the north. The possibility that he was attempting to control the old trade route cannot be ignored. In Hittite times the turbulent Kaska tribes of the Pontic Mountains attacked Hittite territory as far south as Kanesh, Ninasa and on one occasion Hahhum, which if identical with Hahhum is probably to be located at Samsat on the Euphrates south of Malatya.

Military expeditions and trade are always closely linked and in the period that lies between a hypothetical opening of this trade route from Euphrates to Black Sea Coast and recorded campaigns - the Early and Middle Bronze Ages - fall legendary campaigns of the great Mesopotamian conquerors of Agade (themselves of Syrian origin), Sargon and his grandson, Naram-Su'en. The authenticity of these events, long suspected by M. Mellink, is partly confirmed by P. Matthiae's discovery of Ebla and its palace archives at Tell Mardikh, west of Aleppo. The destruction of Ebla is now attributed to Naram-Su'en and a number of other sites like Armanum (Aleppo) and Apisal may have suffered the same fate. This king also left a stela at Pir Husyein, east of Diyarbakir and mentions going beyond Subartu into the mountains. The legends go further and mention that Sargon crossed the Euphrates at Hahhum (Samsat), which was spared, and marched to Purushattum (Acemköy) to settle some difficulties that had arisen between its king (Nur-Daggal?) and Sargon's merchants residing there. Naram-Su'en is said to have been opposed by a coalition of seventeen Anatolian kings, among whom three are mentioned: Pamba of Hatti, Zipani of Kanesh and another Nur-Daggal of Purushattum. In the light of the new evidence from Syria these legends probably contain a germ of truth, if not the whole truth.

Tahsin Özgüç's discoveries in the EB3 layers of the mound of Kültepe-Kanesh lend substance to the historical evidence; here indeed are the remains of substantial buildings (palaces or temples), evident prosperity and trading relations with such areas as had come under the sway of the kings of Agade, albeit temporarily. With the veil of pre-history being lifted from northwest Syria there is new hope that the same may happen soon in the adjacent regions of central and southern Anatolia; evidently more extensive excavations will be necessary. What is already clear is that the historically well attested activities of Assyrian merchants in

those parts during the beginning of the Middle Bronze Age, the 'Karum-period' had EB3 predecessors, during which there were kingdoms with dynasties, of varying size and wealth as is illustrated by such cemeteries as those of Alaca Höyük, Horoztepe, İkiztepe, and a number of lesser ones, all in the northern metalbearing part of Central Anatolia. Their wealth must be derived from the supply of metals to Northern Syria, and through there to Sumer and Akkad. In return came such luxuries as lapis lazuli, now attested in Kültepe 13 and 11a, roughly the equivalents of ED III B and Ur III.

Inevitably the publication of this essay involves chronology anchoring Kültepe to north Syria and Cilicia, in a more secure way than was previously possible. There remains the old problem of Kültepe's links to Western Anatolia, on which there are two views. M. Mellink includes all of Troy II, as well as Troy III-V, in Early Bronze 3; whereas others besides myself have included only the end of Troy II in EB3 and the bulk of Troy II retained in EB2. More simply put: does Troy II begin c.2600 or c.3000 BC, and when does its trade with Kültepe begin? On the face of it, only after EB2, none of the characteristic plates having been found in EB2 levels (p.39) there. The Kültepe evidence seems to confirm what was already known from Tarsus, Semayük-Karatas, Kara Hüyük-Konya, Beycesultan, Aphrodisias, etc.; a surge of wheelmade pottery, rooted in the Troy II tradition spreading over most of western and southern Anatolia at a date, computed to lie after the 10th year of Khufu (Cheops), who ruled on K. Baer's chronology 2638-2613 and independently judged to lie around the transition from EB III A to B in Mesopotamia; ie. around 2600 BC, on the evidence of the Syrian imports at Kültepe.

These events are linked to changes in culture, a decline in the imaginative shapes of earlier and handmade pottery, a greater ceramic uniformity, the introduction of many west Anatolian elements, etc., and political shifts in, eg. the Konya and Cilician plains, and the rise of Central Anatolia probably at the other regions' expense. Many of the great city mounds in the Konya plain were burnt and not rebuilt in EB3 judging by the spread of surface sherds. A shift in trade routes seems indicated between EB2 and 3, and in places there may also have been a change of masters, with new elements coming to the fore, who half a millennium or more later, will emerge in the texts as speakers of Indo-European languages.

These are key problems in Anatolian archaeology that need to be reviewed constantly in the light of new evidence or new insights.

JAMES MELLAART

YAKAR, Jak. *The later prehistory of Anatolia: the Late Chalcolithic and Early Bronze Age* (BAR Int. Ser. 268). 2 vols. Oxford, British Archaeological Reports, 1985. vii + 467 pp., illus. £24.00.

In the author's words 'This book aims at providing a comprehensive picture of life in Late Chalcolithic and Early Bronze Age Anatolia' (p.1). This is, all would agree, a very laudable intention, as no book on this subject exists, and all previous summaries are short, and worse, twenty or more years old. Much new archaeological material is included in the book, and much advice is offered in favour of a more interpretative, rather than a purely descriptive treatment of the material. There is a proper stress on socio-economic patterns, hitherto very much neglected in this period, settlement patterns and the importance of field surveys (the author has conducted some himself) and what they can and cannot reveal. There are interesting speculations on villages, towns, cities and nomad camps, on density of population, etc., refreshingly represented without any dogma and stressed as tentative speculation. The importance of metallurgy and raw resources and Anatolia's environment is duly taken care of and illustrated with numerous maps. Building methods and varieties of houses, construction materials, etc. are duly noted and neatly combined in a chapter. A chapter on skeletons is presented and some of the conclusions drawn about populations are discussed and their value assessed.

All these are steps in the right direction, including a few *faux pas*, easily spotted. Among these is the use of 'Anatolia' for the whole of Turkey, which is of course geographically acceptable, but ambiguous for

Mesopotamian and Syrian cultures in the south east, no respectors of modern frontiers. Migrations caused by rotting fish is another picturesque touch (p.76); immigration of sea-gulls might also be postulated to clean them up.

What I miss in this book is an awareness of the paucity of material evidence for the period concerned. It is admittedly much richer than it was twenty years ago, but in my view, still not strong enough to support some of the author's conclusions based on it. Much material, new then, still remains unpublished; on the other hand, the author is probably conversant with new material of which we are still unaware, eg. from Turkish Thrace, or the Turkish stretch of the Euphrates. Not infrequently strong opinions are expressed without the necessary evidence to back it up, and this alone makes the use of this book for teaching purposes at times hazardous. Having taught since 1961 - and Yakar was a student of mine at Istanbul University long ago, for a while - unsubstantiated statements in the classroom elicit adverse comment and even more so when they appear in print.

This brings one to the author's view of Indo-European immigrations into Anatolia from the beginning of the Late Chalcolithic, eg. at Kurugay H. near Hacilar, which he dates to c.4200-3600 BC, followed by the Beycesultan Late Chalcolithic I-IV, 38/3700 - 33/3200 BC (p.119) on C14 evidence.

As nothing remotely similar to the Late Chalcolithic Kuruçay material has yet been found or published from anywhere in SE Europe, the suggestion that it is in any way related to putative invasions by Indo-Europeans (Kurgan I on M. Gimbutas's scheme) has no basis in fact. The excavator of Kuruçay, Professor R. Duru, believes that the Late Chalcolithic people destroyed the earlier Hacilar I settlement, and settled on top of it; he rejects the low date, which is completely at variance with those we obtained from Hacilar I, which calibrated is c.5500, not 4200 BC. Yakar's dating of the Late Chalcolithic of Anatolia, starting at c.4000/3800 BC puts it in line with the beginning of the Uruk period in Syro-Mesopotamia, which would suggest, if logically followed, that if Uruk equals L.Chalcolithic, Hacilar at Kuruçay is contemporary with the preceding period: Ubaid. For theorists, unaware and oblivious to stratigraphy, the presence of a Syrian(?) Halaf sherd in Hacilar I may be unimportant; to field archaeologists its presence means

that Hacilar I (and the equivalent material from Kuruçay, 5 kms away) is not contemporary with Ubaid, but with Halaf. This is not just a question of dates, whatever they are, but of common sense recognition of similar archaeological evidence - a Halaf sherd from Ras Shamra, Carchemish, or Arpachiye, or Hacilar links these areas into a more or less contemporary period. If Yakar is right in placing the Late Chalcolithic of Kuruçay at c.4200 BC, it implies a linkage with Ubaid 4 (its latest phase in Lower Mesopotamia and with Degirmentepe near Malatya), the archaeological demonstration of which would be not so much a *tour de force*, as a futile exercise in chronological acrobatics. This is of course nothing new; Braidwood dismisses the much earlier neolithic site of Çatal Hüyük as a 'westerly variant of the Halafian tradition'; second rate stuff, presumably, derivative and not original.

Such visionary attitudes to archaeology have long dominated research in the Near East creating myths of superiority with Mesopotamia as the fountain-head of all excellence. The image has worn thin and with new discoveries outside Mesopotamia other cultures also demand our respect. In this work also the old awe has not yet been overcome. Many of the concepts linked to Sumer and Akkad appear to be much more widespread throughout southwest Asia and as long as the huge Early Bronze Age mounds in the Konya plain remain unexcavated arguments about urbanisation or lack of urbanisation remain futile. What is needed is more evidence not less; theory is no substitute for facts, even if 'facts only complicate matters'.

Yakar is a firm believer in Indo-European migrations, often unreasonably so, in my opinion, to account for features and similarities that could also be interpreted through contact and trade, eg. the Ezero culture, now that Özdogan's surveys have shown that Turkish Thrace culturally belonged to Europe (pp.75-6). That I-E elements found their way into Anatolia from across the Bosphorus and Dardanelles is inherently likely, but their influence on the material culture of Anatolia in the period concerned appears to be minimal before EB3 and then only negative. In my opinion the rich cemeteries of EB2 and 3 dynasts are those of native populations, not I-E newcomers.

In an earlier article (*AS*, 1979, p.67) Yakar wrote: 'I agree that calibrated C14 dating, despite its deficiencies, cannot be

totally ignored. But I suggest that all reliable dates, and not only those fitting any particular chronology, should be considered in the process of selection. As it stands now most Anatolian C14 dates do not really favour a chronology as high as the one proposed by Mellaart'.

In this book Yakar reverts to the so-called Middle Chronology; he does not practice what he preached before and opts in general for low dates, including some isolated ones, few other scholars find acceptable (eg. on p.119). And what is a reliable date and on whose authority is it considered reliable? Yakar's attitude to calibrated C14 dating is bizarre; he does not like Bulgarian dates and seems to infer that the system changes across the Turkish frontier (p.79-80). He also has no objection to shifting Beycesultan's Late Chalcolithic 4-2 into West Anatolian EB1 (p.162) or suggesting that at this site the black wares can be confused with EBA wares (p.162). As the excavator of the sequence I can assure him that he is utterly wrong. This sort of criticism is unacceptable and betrays his unfamiliarity with the material. As a matter of fact Yakar listens too often to people whose knowledge of Anatolian material is often only second hand. Unable to discern between valid and invalid opinions he has concocted a chronology of his own that might have been produced by a committee; no firm opinions, only compromise. I shall leave it to others to dispute it in detail, having given one example of how this constipated sequence distorts broader based cultural relations with ludicrous results. The author of course is entitled to his opinions - we do not all have to agree - but to me the book is regrettably spoilt by a faulty chronology. If Yakar regards my dates - which allow a little leeway for expansion - too high, his dates, which do not, are in my opinion too low. Whichever is the greater fault, future generations of archaeologists will judge; only they will have more material at their disposal. What matters at the moment is not the quasi-exact dates, but the construct of contemporaneity and cultural relations that can be deduced from them.

To come back to Yakar's initial date for his IE at Kuruçay and no doubt elsewhere; a date of c.4200/4000 BC would fit in with M. Gimbutas' reconstruction of the First Kurgan ware c.4400/4200 BC. On my dating Hacilar I, if destroyed by the people of L.Chalcolithic Kuruçay, would give a date of c.5500 BC, which in the regions of Southeastern Europe lies

long before the first recognisable presence of any IE elements from the Pontic Steppe. If one does have to search for Karanovo VI-Gumelnitsa refugees in pottery terms, they can be argued to influence Beycesultan Late Chalcolithic 3 and 4 where the white painted decoration may be Anatolia's equivalent of graphite painting in Thrace up to the Sea of Marmara. In anybody's chronology, Yakar's excepted, well before any Indo-European movements into Anatolia.

Summing up, Yakar's book contains much that is of great interest and a mass of very useful illustrations, but alas, forced into what I would call an overtight and unconvincing chronological straightjacket.

JAMES MELLAART

McNICOLL, Anthony. *Taskun Kale: Keban rescue excavations, Eastern Anatolia* (British Institute of Archaeology at Ankara, Monograph No. 6) (BAR Int. Ser. 168). Oxford, British Archaeological Reports, 1983. 266 pp., 130 figs., 40 pls.
£15.00.

In the early 1970's the British Institute of Archaeology at Ankara moved into the international rescue archaeology project launched by the Middle Eastern Technical University on the upper reaches of the Euphrates river in eastern Turkey in advance of the construction of a massive dam and a huge man-made lake. David French, the Institute's Director, planned and directed the British contribution under the name of the Aşvan Project, a multi-disciplinary field enterprise which aimed at recovering broad-spectrum information about the whole chronological range of the area's settlement history through the medium of investigating a pair of neighbouring sites within the framework of their physical environment and ethno-historical context. Stephen Mitchell produced the first of the excavation reports on Aşvan Kale itself in 1980; and Tony McNicoll, who died tragically young in 1985 after a long illness, lived long enough to see the publi-

cation of his report on the excavation at the site of Taskun Kale, three kilometres up the valley of a tributary stream.

The site of Taskun Kale was occupied first in the Early Bronze Age in the third millennium BC, again in late Hellenistic and Roman times, and finally by a 10-hectare settlement of the 14th century AD, bi-focussed on its church and on the fort which gave the site its modern name. Very little of the middle period of occupation survived to be excavated after being exposed to centuries of erosion and then levelled and terraced at the construction of the fort. And the Early Bronze Age material, reached only in a sounding, is to be treated separately in another volume devoted to the remains of that period from the whole project. Thus most of this report is concerned with the final, short phase of settlement, and in particular with the fort and the church. In the words of the author, this volume is 'a basic, skeletal record of the dig, with a minimum of interpretation'. However, there is good reason to reject that over-modest assessment: although certain specialist reports (eg. on the physical environment and the prosecution of agriculture) will appear in later volumes, the report is much more than 'basic' or 'skeletal', and its level of carefully segregated, intelligent interpretation is much more than minimal. Unmentioned in the author's prefatory remarks quoted above is another aspect of the report, the necessary (but rarely seen) exposition of the methodology of excavation and recording balanced by a fascinating and totally candid review in the light of experience of the limitations, shortcomings and faults of the chosen methods.

The excavation report proper deals with the work on the kale itself, and with the second area of concentration some 150 metres away, where a contemporary Christian church, built on the site of a much earlier church, was explored. The following sections then describe and discuss the glazed and unglazed medieval pottery, the pottery of the classical and early Christian phase, and the small finds, which consist of coins, jewellery, a few metal objects, some faience, glass and five stones with simply carved crosses. Tony McNicoll's work closes with an admirably concise four pages of conclusions. This is followed by an essay in the spatial analysis of the fort by Roland Fletcher, in which he identifies signs of regularity in the spatial ordering of features, which then tends to dissolve into disorder. There is also a short series of

appendices of tabulated information, but including one by Sebastian Brock on the little group of fragmentary Semitic funerary inscriptions. As well as the standard archaeological illustrations of plans, sections and artefacts, there are helpful and illuminating explanatory diagrams and isometric semi-reconstruction drawings of the building complexes. The standard of reproduction of the illustrations, which were certainly drawn to appropriate scales with appropriate pens, is disappointing; the results are frequently blurred, and much detail has been blotted and blacked out in the printing process.

The extent of the settlement around the kale was determined from the surface scatter of pottery, and practically no time could be devoted to its excavation. The rebuilt church and its attendant graveyard would seem to have been the religious centre of the settlement. The kale was an oval enclosure with rectangular bastions and a single entrance; all around the interior of the enceinte wall was disposed a range of 30-odd rooms, and at the centre is an irregular, roughly oval courtyard. Many of the rooms showed indications of use as living quarters of the putative garrison.

McNicoll points to the abandonment of the agriculturally preferable site of Aşvan, which was also strategically sited at the Euphrates crossing, while Taskun was briefly occupied, and to Taskun Kale's situation controlling the road between the river crossing and ancient Harput (modern Elazığ). He infers that the fortress was ill-equipped to withstand attack or siege, and too small to serve as a refuge for the villagers. In view of the historical situation in the early 14th century, when the Ilkhanid dynasty was asserting its authority through eastern Turkey, he argues persuasively for the interpretation of Taskun as the compulsory and short-lived resettlement of the Christian inhabitants of Aşvan under the watchful eye of a gendarme-like garrison in the kale. Not surprisingly, there are no known parallels for such a site, because no-one has undertaken this kind of archaeological project before and excavated such sites. The demands of rescue archaeology concentrated attention in a part of Turkey which had not previously drawn archaeologists; but the British Institute's project must take the credit for the fertile way in which the new ground has been broken.

TREVOR WATKINS

SAGHIEH, Muntaha. *Byblos in the Third Millennium BC: a reconstruction of the stratigraphy and a study of the cultural connections*. Warminster, Aris & Phillips, 1983. 147 pp., 80 figs., diags., pls. £28.00.

Byblos, on the coast of the Lebanon to the north of Beirut, where digging began in 1924, has long been known as a place of an importance quite disproportionate with its relatively small size. In the eyes of Egyptian pharaohs of the Archaic and Old Kingdom dynasties the Lebanon was a vital source of timber, now a mundane staple of ordinary industries, but then a strategic material essential for temple or palace building as well as the construction of large, sea-going ships. Egyptian favour towards Byblos is evidenced by the unsurpassed number of prestige objects inscribed with the names of a long series of pharaohs. The Levantine coastal cities were also of considerable importance to the equally ancient kingdoms of southern Mesopotamia, although direct contact was more difficult for them. In the eyes of modern archaeologists Byblos ought to hold the keys to unlocking the cultural and chronological correlations between the well-worked southern Levant and the much more reticent but tantalising Early Bronze Age crossroads of Syria. Yet, despite copious volumes of *Fouilles de Byblos*, we know very little about Byblos or its excavations, because the excavation and publication strategies of the French expedition scarcely seem designed to elucidate.

Muntaha Saghiel's book is produced from her Institute thesis, retyped single-spaced and photo-reduced onto a long, narrow page with a broader outer margin, in which 'footnotes' are set alongside rather than under the text. The unusually large overall dimensions of the book, which is well above A4 size, do not make it easy to handle. A few pages at the beginning and end, such as the title-page, the tables of contents and bibliography, have been conventionally typeset. All the illustrative material is concentrated at the rear of the volume, the bulk consisting of schematic 'sections' reconstructed by the author to run through the building complexes, accompanied by tiny plans which serve to label walls and locate sections, and thumbnail sketches of objects. The figure numbers are buried in the body of each such illustration, which does not make them easy to work

with; and objects referred to in the text may only be located in the illustrations by noting in which phase of which building they are reported in the text and then finding the figure which records the reconstructed sections for that building. The author's own drawings of some of the pottery studied in Beirut are of a very much better standard than the strange little sketches reproduced from the excavator's report.

The author's main task, and the one on which most time and attention were clearly expended, is the attempt to assemble from the available records a structural history and stratigraphic analysis of the third millennium material. This lengthy first chapter, which in fact occupies more than half of the whole book, tests to destruction the contention of the excavator that, despite the acknowledged deficiencies of the excavation and a publication devoid of even the lowest level of synthesis, '... nous présentons aussi au lecteur tous les moyens de contrôlé et de recherche pour se faire une opinion originale'. Muntaha Saghiel can at best give heightings on the tops and bottoms of walls. Everything else (except where there was a stone-paved floor) was removed in arbitrary, horizontal, 0.20 m spits, which were also referred to by their heights. Selected finds (whole pots, bronzes, stone vases) were kept and recorded by reference to the spit and the grid-rectangle (no squares at Byblos). Major fire destruction horizons were also noted.

From this meagre information Muntaha Saghiel is able to put (some) objects back inside buildings and relate their arbitrary spit to some walls. In the end, however, as the author frequently has cause to remark in her phase by phase discussion of building after building in area after area, the analysis reveals unpleasant lacunae and serious discrepancies which can only very occasionally be resolved, and that, alas, hypothetically at best. While Dunand's own discussions of important building complexes can be shown to be inconsistent with the published facts and changeable over the years, Muntaha Saghiel's reinterpretations seem eminently reasoned and often preferable, but remain ultimately untestable. None of the crunch questions involving dateable objects in buildings can be answered definitively, simply because there were no stratigraphic or structural relationships noted between the spits within which artefacts were recorded and the vertical stone structures. One

thing which is most succinctly demonstrated is that, in any case, many of the stone vessels inscribed with the names of Egyptian pharaohs must have been found in contexts many centuries removed from their date of manufacture.

It is perhaps disappointing that no general synthesis of the city of Byblos and its important role as a commercial and cultural intermediary over the third millennium BC is attempted. The author remains strictly concerned with a lower level of synthesis. Having completed her analysis of the structural history and reconstructed things somewhat within the general periodisation proposed by the French, the rest of the work is concerned with working out the chronological range of each phase within the French excavator's periods K and J phases I and II (the basal period L is omitted on the grounds that it is not of the third millennium). Not even the important foreign contacts are pursued beyond the matter of the chronological light they may shed. Chapter II gives a brief and simple review of the pottery, in part based on original study of the material held in Beirut, and Chapter II considers the chronological implications of the pottery in terms of links with Egypt, Palestine and Syria (and the wider world of Anatolia and Mesopotamia to some extent). The author asserts that there is no evidence for contacts between Byblos and Egypt before a certain stage in Dynasty I, contrary to the views of Helen Kantor, which have been sanctified by age and repetition. In this she is contradicted by K. Prag's analysis of the links between fourth millennium Byblos and Egypt in *Levant* 18 (1986). In terms of the correlation with Palestinian Early Bronze Age chronology, which has been so much more thoroughly documented and analysed, there is some inconsistency in Miss Saghiel's conclusions between different parts of the text and between text and the tabulation of the conclusions on p.109. It is also an unfortunate accident of the timing of the two publications that she has not been able to take account of the massive review of EB pottery in the Jericho final publication (or other recent important developments elsewhere).

Chapter IV summarily surveys temple architecture throughout the Near East in the search for parallels which might have chronological significance. The final chapter of conclusions and discussion is indicatively brief, in part because the author has set herself a specific task which excludes general

discussion of the role and significance of Byblos in the third millennium, but most necessarily because the conclusions yielded by the massive input of the author's labour cannot in the end make up for the horrifying inadequacy of the old excavations.

TREVOR WATKINS

MUHESSEN, Sultan. *L'Acheuléen récent évolué de Syrie* (Bar Int. Ser. 248). Oxford, British Archaeological Reports, 1985. viii + 261 pp., 93 figs., 9 maps, 27 tables. £15.00.

The author, as a Syrian prehistorian, is well-placed to write on his subject - a new aspect of the Lower Palaeolithic lithic industries of Syria. Having taken his first degree in prehistory at Warsaw University under Professors Chmielewsky and Koslowsky, he was employed by the Syrian Department of Antiquities, taking part in the fieldwork, excavations and surveys which were carried out by various foreign expeditions. He became a member of a French CNRS team who were studying the Palaeolithic and its geomorphology in the Levant between 1978 and 1981: he was joint director of the excavations at an important Acheulean site located on their surveys. Independently he also undertook a study of the Lower Palaeolithic of a little known area in south-western Syria. He presented his dissertation to the University of Lyon in 1982, and this volume is an amended and expanded version of that work.

The title is based on the discovery that a separate phase within the Late Acheulean of Syria could be distinguished, both typologically and on geomorphic and chronological evidence. This phase is regarded as an advance on the earliest Late Acheulean industries (known to be incorporated in 'Rissian' river terraces) while preceding the Final Acheulean complex; the latter seems to have occurred as late as the Last Interglacial and a variant is known as the Acheuleo-Yabrudian, e.g. at the well-known cave sites, Tabun, Yabroud and Bezez.

The author discusses the geographic and geomorphic framework, the history of Lower Palaeolithic studies in the area, the chronology of the handaxe-using Acheulean cultures in four (Early, Middle, Late and Final phases), as known in Syria, and describes the basic data available for research. He then presents, in a clear and well-reasoned way, the evidence for the last-but-one industrial phase (the Evolved Late Acheulean), based on his research in inland and coastal regions. The work is plentifully illustrated and there is a comprehensive bibliography.

As the author freely admits, not all the answers to the problems of the Lower Palaeolithic succession are yet to hand, and absolute dates, as well as faunal and pollen evidence, are still lacking. Nevertheless, it is a step forward to be able to use the (even if few) archaeologically and geomorphically *in situ* sites (such as Latamne, Gharmashi Ib, Hamman Kebir, Yabroud) as a basis for 'placing' the large number of surface assemblages, in order to construct a reasonable chronology for the Acheulean of Syria as a whole.

Little further similar work has been done since this volume appeared, so that it should form an indispensable reference for this period in the area for some time to come.

L. COPELAND

SUSSMAN, Varda. *Ornamented Jewish oil-lamps: from the destruction of the Second Temple to the Bar-Kokhba revolt*. Warminster, Aris & Phillips, 1982. (First published in Hebrew by the Bialik Institute and the Israel Exploration Society, Jerusalem, 1972.) xi + 136 pp., illus. £20.00.

This book, based on a shorter Hebrew version, deals with a group of lamps made in Roman Palestine by Jewish artisans, and which are contemporary with the later 'Herodian' lamps. Though similar in shape they differ from these latter in being mould rather than wheel made, and by bearing decoration in delicate relief. They have been found largely in

the region of Beth Guvrin (Beit Jibrin) south-west of Jerusalem: the authoress has accordingly named them 'Darom lamps', Darom being the name used in contemporary Jewish sources for the Beth Guvrin-Hebron region. The book is based on the study both of lamps in the collection of the Israel Department of Museums and Antiquities, and of others privately owned; lamps and lamp fragments found by Professor E. Oren in his excavations at Marissa near Beth Guvrin are also taken into consideration. A sizeable proportion of the total number of such lamps known is presented and analysed, and a number of sub-types in the decoration is established. Detailed study of typological variants in lamp shapes is not undertaken, though the bibliography on p.viii could be used, presumably, by readers interested in these for guidance. The book is in fact intended for those interested in Jewish art as well as archaeologists, and may appeal to the former in particular.

In view of this special slant, the writer offers in her first chapter an outline of the development of the oil lamp in ancient Israel, from the earliest times down to the Arab period, which will help non-archaeologists to see the matter dealt with in its proper frame of reference. In addition, a number of other relevant topics are briefly dealt with here which serve to indicate the place of the oil lamp in the Jewish world of the time, and to introduce other contemporary or slightly later Palestinian oil lamp types which are related to the 'Darom' lamps. Chapter II begins with a brief introduction outlining the religious reasons why Jewish potters shunned, with few exceptions, the representation of living creatures, and those of human beings totally; instead they decorated their lamps with floral or geometrical motifs executed in a fine linear style, or with representations of objects of religious significance in particular, such as the *menorah* (seven-branched candelabrum). Attention is drawn to the relation between this decoration and that found in other contemporary Jewish art, especially on stone ossuaries; indeed, the writer thinks the same craftsmen may have sometimes worked in both fields. Chapter III lists, and discusses in some detail, the various decorative motifs shown on the lamps. These include in particular a number of Jewish emblems used widely in the period, besides the *menorah*, objects of a secular nature which might however have evoked religious associations in the mind of Jewish

customers, but also purely secular motifs like ear rings, and floral and geometric patterns. In the Catalogue which follows the lamps dealt with are presented individually by excellent photographs with accompanying text. The length and width of each item is given, the decoration is analysed in detail, and relevant artistic parallels are noted. (It might have been useful to add find spots and more exact archaeological datings where these are known.)

The book presents its subject clearly, and offers a great deal of information in a succinct fashion. The references to the contemporary religious background which determined the artistic repertoire of the potters will be particularly enlightening to readers unfamiliar with that subject, and the systematic guidance to analogies in contemporary Jewish applied art in other fields is similarly helpful.

A few misprints apt to cause trouble to readers should be corrected in a future edition. On p.23 the oil lamp figured is not no. 42 in the catalogue but no. 45; similarly on p.24 not no. 51 but no. 55 seems intended, and on p.26 not no. 206 but no. 76; and what equivalent in the catalogue is there for the alleged no. 220 on p.27?

The book, the fruit of much labour and which brings together and analyses a body of evidence not previously treated together, will be of real service to historians of ancient Jewish folk art in particular, but also useful to others concerned with the arts and crafts of the Roman and Byzantine periods. Mrs Sussman deserves their thanks.

B. S. J. ISSERLIN

17 figs., 1 map, 3 pls. £33.00.

Aurel Stein was famous as an explorer and orientalist long before he tackled the Roman world in his later years. His new interest lay in searching for links and analogies, historical and topographical, between Rome's eastern frontiers and trade routes, and those of the great oriental powers of contemporary date.

The publication of Stein's *Limes Report*, left in typescript at his death over forty years ago, has long been awaited by scholars. In these volumes, however, we have an archaeological story beginning in the early years of scientific exploration and continuing to our own day.

Stein's own survey forms, of course, the major part of this story. He discussed the topography he observed with erudite relation to the ancient sources, as well as more recent archaeological information. The breadth of his survey is equally important. Covering an area from eastern Iraq to southern Jordan he was complementing the survey of Roman Syria published by Poidebard in 1934. Although Stein attributed a Roman date to most remains which he thought classically appropriate, and some areas are based on second hand knowledge, a more or less complete picture of this stretch of the Limes was built up. Despite the Roman nature of the work, Stein, fired by topographical discoveries, also made acute observations on the campaigns of Alexander the Great.

Like Poidebard before him Stein made extensive use of aerial reconnaissance and photography, as well as visiting most sites on the ground. The importance of this is noted by the editors who observe that it is '... impossible for archaeologists to travel in these areas (today) with the same casual ease ... flying has been out of the question and aerial photographs are usually not obtainable...'. The publication of many of Stein's photographs and plans, along with new ones, forms an important contribution to these volumes. With the commentary on the *Limes Report*, this archaeological story has been brought up to the present. The sites noted by Stein are here discussed further in the light of subsequent research, accompanied by a full bibliography. The light to be shed upon research by past investigations forms an important part of the archaeological

STEIN, Sir Aurel. *Sir Aurel Stein's 'Limes report': the full text of M. A. Stein's unpublished 'Limes report' (his aerial and ground reconnaissances in Iraq and Transjordan in 1938-39); edited with a commentary and bibliography by Shelagh Gregory and David Kennedy (BAR Int. Ser. 272).* 2 vols. Oxford, British Archaeological Reports, 1985. xxi + 368 pp., 37 figs., 2 maps, 72 pls.; 122 pp.,

repertoire, a repertoire now enriched by the publication of Sir Aurel Stein's Limes Report.

JULIAN BOWSHER

stretches back to the time of Alexander at least. However, this omission and the minor mistakes in the English should not detract from as comprehensive a study as we are likely to get of building technology in Nuristan.

JULIAN BOWSHER

EDELBERG, Lennart. *Nuristani buildings* (Jutland Archaeological Society Publications, 18). Aarhus, Jysk Arkæologisk Selskab, Moesgård, 1984. xxvii + 223 pp., 1 map, 227 figs. Price not stated.

Nuristan is a small but distinct area of eastern Afghanistan. The late Lennart Edelberg had spent over thirty years studying the peoples and cultures of this area. In this posthumous work the subject under scrutiny is the architecture, more properly, the wooden architecture, and associated woodwork. The bulk of the book is dedicated to the domestic house, but there are chapters on graves, stables, barns, bridges and mills.

It is a technical work which analyses each aspect of construction, not only in terms of structural elements, but also the tools, terminology and costs of construction. There are many case studies of buildings known to the author as well as generic discussions. The profuse illustrations include line drawings which clearly reveal the plans and structural details of individual buildings. There are many photographs, in colour and black and white, taken over the last forty years. After a location map at the front and a larger scale map of Nuristan at the back, it is refreshing and useful to find many thumbnail maps scattered throughout the margins which identify all the sites mentioned in the text.

There is an important ethnographic element throughout the book. Edelberg has been careful to remark on the linguistic and social, as well as the geographical, variations illustrated in the area. Edelberg's ethnographic and architectural evidence goes back to the earliest studies in the area, in the last years of the nineteenth century. It is a pity, though, that there is no discussion on the historical tradition of building in an area that has an archaeological heritage that

HART, G. *A dictionary of Egyptian gods and goddesses*. London, Boston and Henley, Routledge & Kegan Paul, 1986. xv + 229 pp., 80 figs., 2 maps. £12.95 (hardback); £5.95 (paperback).

In what is virtually a pocket dictionary the author describes most of the Egyptian deities including several fairly obscure ones and a few of Asiatic origin which were worshipped in Egypt. Not all are, however, given separate entries, some being mentioned only as associates of other gods, so that in the absence of a comprehensive index the book loses some practical usefulness. The scale has precluded any sort of documentation apart from a single page of select bibliography. This results at times in vagueness. The first entry, for example, reads simply: 'AKEN. The custodian of the ferry-boat in the Underworld. Rather amusingly he has to be woken from slumber by the ferryman Mahaf to provide the boat for travel on celestial waters'. It might have been mentioned that the source of this information is Coffin Text spells 343 and 397. If the reader does not need to know where this very minor character appears, he will not need the explanation anyway.

The book is agreeably written with the general public in mind, and contains much to interest the browser. Whether in this compressed form it will shed much light is another matter.

H. M. STEWART

SCHÄFER, H. *Principles of Egyptian Art* ;
(edited by E. Brunner-Traut, translated
by J. Baines). Oxford, Griffith Institute,
reprinted with revisions, 1986.
xxviii + 470 pp., 109 pls., 331 figs.
£30.00.

Schäfer's classic grammar of Egyptian art, first published in 1919, went through four German editions, the last of which, left unfinished at the author's death, was edited by Emma Brunner-Traut and issued in 1963. The present English translation of that edition by the Professor of Egyptology at Oxford first appeared in 1974, and is now reprinted with addenda and corrigenda, mainly up-dating the textual notes. The illustrations remain unaltered. Most welcome in the translator's introduction are the list of reviews of the 1974 edition and subsequent bibliography, which serve to carry the discussion forward.

H. M. STEWART

delicate, and a smithy. This last was thought to be a lead smelter which was thought to be a rather surprising plant to be found in the centre of a town. But it is clear from the slag analysis that this was not the case. These slags date from the 3rd century BC and have close affinities with those from Carthage.

R. F. TYLECOTE

LANCEL, Serge (ed.). *Actes du IIe colloque international sur l'histoire et l'archéologie de l'Afrique du Nord réuni dans le cadre du 108e Congrès national des Sociétés savantes* (Grenoble, 5-9 avril 1983) (*Bulletin archéologique du CTHS. Nov. sér. fasc. B. Afrique du Nord*, no. 19: année 1982-3). Paris, Comité du Travaux Historiques et Scientifiques, 1983. ix + 540 pp., illus. Fr.400.00.

A multi-lingual work on North Africa with 44 contributions, the coverage is mainly artefactual and considers amphorae, ceramics, farms, oil, presses and mosaics. Other subjects dealt with are the economy of Carthage, Italian merchants in Mauretania, Vandals, Romans, Islam, Byzantines. Interesting items are the Venetian coral trade, and the problem of whether the Carthaginians reached the Azores. One paper discusses the economic life of the town of Thysdrus near the coast of Tunisia. Here there was a workshop for the manufacture of bone pins - very small and

CLARK, J. Desmond and BRANDT, Steven A. (eds.). *From hunters to farmers: the causes and consequences of food production in Africa*. Berkeley, Los Angeles and London, University of California Press, 1984. xii + 433 pp., illus. £44.00.

This work derives from a symposium held by the American Anthropological Association in 1978 and represents '... the current state of research into Late Quaternary human/plant/animal relationships and the processes leading to food production in Africa' (p.1). In some thirty contributions the writers range through many parts of Africa and times from the Pleistocene to the present. The book is handsomely produced with a useful bibliography, many handy maps and tables, and hardly any noticeable printer's errors. Jargon is not always absent and, if a subjective impression be allowed, it is mostly the anthropologists who score rather highly here, but on the whole it is an easy enough read.

Many interesting observations emerge. For example, Kalahari foragers may have a better balanced diet than the sedentary food producers of the area, especially in times of scarcity; or, Kalahari Basarwa who are, apparently, newcomers to cultivation may have more success with their few crops than long established farmers of the same region (partly because the newcomers seem to be more selective about the species they use and more careful about planting them); or, the consumption of palm oil, rich in vitamin A, gives some measure of protection against

'river blindness' (onchocerciasis) in the West African areas where it is endemic. It becomes obvious that simple, blanket explanations for the processes of transition to settled farming in the various localities can no longer suffice and that the special complexities of each must also be considered.

Similarly, with lactose intolerance of particular human populations - mentioned several times in the text but not examined in any detail for its adaptive implications - clearly this would influence relationships between human beings and the animals they might or might not rear for dairy products. Another dietary detail - not mentioned in the book - that could be relevant to the discussion is the effect of combining pulses and grains; apparently, these food items are more nutritionally effective when combined in a meal than when taken separately.

As regards interpretation, notes of judicious caution are raised throughout the book: such as the frequency with which plants have been misidentified in the literature; the many and great difficulties the palynologists have in identifying African plant material; the near impossibility of confidently separating domestic bovids from similar wild forms in the existing incomplete bone records; the dangers of interpreting scant remains from a small temporary camp while a more permanent settlement site of the same society may exist elsewhere; or, how the history of Khoikhoi hunters' transition to herding may be coloured by archaeological finds of domesticated livestock in rock shelters, which in reality may be the fruits of thefts by the hunters.

Two of the writers were told by Kalahari Kūa that they did not wish to grow food because 'It's too damned much work'.(p.341)! And, another writer warns '... our time, money and human resources will probably never be any greater than they are now' (p.251). All this and much more makes this a well informed and absorbing collection of papers.

ALEX HOOPER

HERBERT, Eugenia W. *Red gold of Africa: Copper in precolonial history and culture.* Madison, University of Wisconsin Press, 1984. xxiii + 413 pp., 40 figs., 6 pls., 4 maps, 3 charts, 7 tables, 5 diagrams. \$32.00.

In the last few years many of the long held views on the development of metallurgy south of the Sahara have begun to be challenged by new discoveries and dates from all over the continent. The work of French archaeologists in the Sahel region, especially southern Mauretania and in the south of Niger, has revealed the remains of extensive copper production with dates stretching back into the second millennium BC. The work of Bernus and Echard at Azelik, Marandet in the Agades region of southern Niger has revealed thousands upon thousands of small conical crucibles. Some were used to amalgamate prills of native copper collected locally. These are probably medieval, but copper working is much earlier, dating back well into the 1st millennium BC. Other crucibles, including some examined by this reviewer, contained a beautiful lead-copper glass - the familiar sealing wax red of the celtic enamels. This has been claimed as the remnants of a sophisticated purification process but it seems more likely that bead making was also carried on, presumably with glass imported from across the Sahara - the sites do have a long history up to the medieval period.

More recently Grébenart has published a quite remarkable series of structures claimed as smelting furnaces from Afunfun, also in the Agades area. Some are undoubtedly smelting furnaces, remains of well vitrified and slagged clay shaft furnaces with abundant evidence of slag, dating from the 2nd millennium BC, but others are strange affairs of groups of irregular tunnels penetrating some metres into the ground. They too are claimed as furnaces but it is difficult to understand how they could possibly have operated, let alone have been constructed. Although the sides are well burnt and charcoal is everywhere, there is no slag associated with them. Surely they are the burnt out remains of old tree stumps, probably associated with the charcoal production which must have taken place alongside the copper smelting.

Thus it does now seem clear that copper smelting south of the Sahara is now firmly established with Afunfun dating to the 2nd

millennium BC, and sites such as in Mauretania and Azelik in full production early in the 1st millennium BC.

However, excavations all over West Africa and the Sahara show equally clearly that copper and its alloys were not extensively used and thus we may still properly say that Africa did not have a Bronze Age in the sense it is understood elsewhere. In fact, the earliest substantial copper-base artefacts are still the bronzes from Igbu-Ukwu in Nigeria where many hundreds of copper, bronze and leaded bronze castings were excavated from a sumptuous burial dated to about the 10th or 11th centuries AD. The technology and the metal itself have both been regarded as imports to the region because of the lack of antecedents - which is justifiable, but also because of doubts over the technical capabilities of the Africans - which is not. The long local tradition of iron smelting and working, which is at least as difficult as copper smelting, together with the belated discovery that copper was being smelted after all south of the Sahara should dispel doubts over ability to make the metal itself. Recent technical studies on the composition, and on the casting technology of the bronzes themselves by the reviewer should also convince everyone that these remarkable bronzes which stand at the head of all African metalwork are wholly African. To their credit both their excavator, Thurston Shaw, and Eugenia Herbert argued the case for local manufacture, but probably with imported metal.

The overriding achievement of Herbert's book is to bring together all the disparate evidence, some new, some old, for indigenous copper making, distribution and use in Africa from the earliest times up until the end of the last century into one coherent account. The overall treatment is first rate - this is a book the subject has long needed and we must be grateful that the work is so complete, informed and yet so readable. With this book it will at last be possible for those studying metal technology to integrate Africa into the general scheme of the development of metallurgy. Clearly it will be much easier and more convincing now we do not have to try and explain how iron working flourished at least as early as in Europe, yet somehow the Africans lacked the skill or initiative to work copper. Those in the field of African archaeology will find this book invaluable for integrating the story of metallurgy into the framework of African history and especially for showing very clearly the crucial role played

by copper in African society, economy and ritual. Thirdly, economic historians will also find this book invaluable for documenting the immense metals trade that developed between Europe and Africa from the late fifteenth century on, successively controlled by the Portuguese, the Dutch and finally the British.

The book is divided into three principal parts, along the lines described above, metallurgy, trade and society. This reviewer is most at home in the first of these topics, whereas the author is probably more at home in the latter two, but the treatment of copper resources, mining and smelting technology and metalworking given here is quite excellent. The author has clearly put in an immense amount of background reading into the whole book and nowhere does this show more clearly than in the metallurgy sections. It is also a considerable relief to report that here this voluminous reading by someone on the periphery of the subject has been carefully and critically assimilated. Thus, for example, she has strong reservations about the value of trace element analysis of copper base artefacts as a valid means of assigning them to any particular grouping.

There are detailed descriptions of early mines and furnaces based upon early travellers' accounts, field surveys and excavation, for sadly, traditional copper smelting had died out everywhere in Africa by the beginning of this century. Pride of place amongst these descriptions must go to the filmed account made by Monsignor de Hemptinene, Apostolic Bishop of Katanga, who mobilised an entire village of the Yeke people to conduct a campaign of mining and smelting using the traditional methods before their memory was forgotten. The film he made must be amongst the most precious documents of early metallurgy. This and other accounts from all over Africa are used to document and describe the exploitation of copper. The survival of traditional methods so recently - within the experience or living memory of those questioned - is of course extremely useful for giving an insight into areas that are less tangible archaeologically, such as the social status of the miners in periods of the remote past. Thus, for example, Herbert is sceptical about the use of slave labour in African metallurgy and makes the very valid point that smelting is a highly specialised job. This seems eminent common sense, and re-

inforces doubts about how common the use of forced labour ever was in mining or smelting. One suspects the miners were not only free but highly trained and rewarded. The extraction of usable ore from deep workings is a highly skilled task and in the very confined spaces of early mines there was just no room for people who did not know what they were about. The ethnographic parallels given in this book on this topic are salutary, as are the comments on the role of itinerant smiths in African metalworking and society.

In West Africa the local production of copper had ceased long before the arrival of the first Europeans, probably as a direct result of Arab competition from across the Sahara. Tin is rather more puzzling. It was produced on a considerable scale during the 19th century right up until the arrival of modern mining companies in the early years of this century. Previously the trade had been almost exclusively in the hands of the Arabs who took the metal north across the Sahara. There are records of a trade conducted from Kano in northern Nigeria in the 1820s, but how much earlier did this trans-Saharan trade exist? Islamic writers in Egypt and the Middle East of the medieval period list their sources of tin in some detail; South East Asia, North East Asia and Europe are all mentioned, but there is no mention of African tin. It is difficult to understand how the Arabs who were so active in their search for gold, ivory and slaves in West Africa could have missed another valuable commodity, tin, if it had been in production.

The establishment of the European maritime trade changed everything, and not just for Africa. Copper and brass were imported on a prodigious scale - the quantities are recorded in detail. Thus, for example, between 1504 and 1507 the factor at Sao Jorge da Mina received no less than 287,813 manillas of copper and brass. Nor was this all: 1582 shaving bowls, 520 urinals, 3192 chamber pots were also included at just this one port over the same period. These quantities are repeated at forts and stations all along the coast. Not only had Africa never seen trade on this scale before; all over Europe mines and metalworks strained to meet the demand. Anyone familiar with inventories taken from late medieval households where the one brass pot was one of the most valued possessions will realise the impact this flood of manufacture must have had on European production, with, of course, the knock-on effect on the development of capitalism and technology,

culminating in Europe with the Industrial Revolution.

Herbert shows the very central place held by copper and brass in this trade. It was the one commodity above all others that the Africans wished to obtain; gold was held in less esteem. Much, if not most, of the copper and brass was needed as a source of tangible wealth, either as manillas or ingots, or in the more visible forms of the often monstrously heavy jewellery and personal adornments the wives of rich chiefs were obliged to wear. As a complement to this, one of the principal attractions of Africa to the outside world was gold - and this metal always played a significant role in trade, although probably not to the overriding extent suggested by Herbert. It is in the final section of Herbert's book where this true role of copper in African society is documented in detail, and the conclusion reached that its role was very similar to that of gold elsewhere. One does leave the remarkably well documented chapter on this two way metal trade with the distinct impression of the futility of it all - the enormous expenditure of effort and skill to win these metals, both copper and gold, just to satisfy the desire for hoardable wealth.

This book deals with many areas of scholarship, some of which, such as early metallurgy, and much later the growth of long distance maritime trade, are going through substantial change, not least though some of the new evidence reported in this book as a result of excavation and research in Africa itself. The sheer quantity of information and its judicious assessment give this book immense authority. What a relief that it is handled so lightly by the author, making the whole considerable volume an absorbing read. This must be one of the best books to have been written on Africa, and one which will be of great value to scholars in a wide range of other disciplines.

P. T. CRADDOCK

BEAUDRY, Marilyn P. *Ceramic production and distribution in the southeastern Maya periphery: late Classic serving vessels* (BAR Int. Ser. 203). Oxford, British Archaeological Reports, 1984. xi + 335 pp., 34 figs., 77 tables. £16.00.

There is a type of very beautiful polychrome pottery called Copador, made and used in the Mayan region between 650 and 850 AD. Beaudry's purposes are: 1) to see if all Copador pottery was made in the same place, and to pinpoint the location; 2) to decide if it was made by craft specialists working within a production system geared to standardisation and simplification; and 3) to see if the size of the production system related to a complex exchange system.

Each objective is dealt with in turn, first by providing a theoretical framework, then by detailed analysis, and then by decoding the results in terms of the theory and reviewing progress. The overall organisation is admirable; and there are frequent, lucid summaries which pick up all the threads and carry them on to the final chapter, where they are woven into a useful review of Mayan economic and cultural development.

Unfortunately, the lay-out is often confusing: it would be better to banish tables and footnotes from the main text, unless a difference in type-face could be made. There are some type-setting errors (pp.12 and 14 are duplicates); and there is the odd inconsistency in Beaudry's use of tenses, when describing existing evidence versus past behaviour (eg. p.21).

But as for Beaudry's three objectives and their resolutions, I have more some more serious qualms. The first objective is impeccable, of course, but is NAA (Neutron Activation Analysis) the proper way to reach it? No Copan valley clays were analysed, and there was no study of the coarse composition of the fabrics. The other objectives depend, at least in part, on these first results: and by means of this analysis the case is not proven, either for a Copan valley provenance, or for a definable number of production units. Beaudry admits this tacitly, and, in the end, relies on the abundance of Copador in the Copan valley to suggest that it was made there.

The second and third objectives draw their theoretical basis from Rice (1981;

1982), who links the measurement of ceramic attributes and distributions to craft specialisation, mode of production, marketing network and status of the consumer: thereby solving most of our problems of ceramic interpretations, if we care to accept the theory. Welbourn (1985) does not, and I agree with his opinions.

Beaudry's section on 'standardisation' and 'simplification' seems to me ill-conceived. For one type of pottery one recognises, by definition, a degree of uniformity, and the shapes and decoration may well be simple. But the terms 'standardisation' and 'simplification' imply the existence of some other, heterogeneous and complex standard of comparison. And even if one accepts that standardisation and simplification are deliberate 'goals' of production, rather than by-products, the measurement of these qualities (and that of 'technical competence') as quotients of, for example, degrees of specialisation, seems quite implausible. In 1981 Rye said '... the classificatory and control devices used by potters in the past have usually been far less precise than our facility for analysing their results'. Here measurement overwhelms a few simple points, and, contrary to the aims of such an analytical approach, taxes the suspension of disbelief.

Nonetheless, Beaudry wields the theoretical and analytical tools with competence, and it all comes right in the end. The analyses do, at least, define the style, and one is left with a clear idea of the pottery and of its importance in Mayan studies. The conclusions are, mercifully, archaeological, not methodological.

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SARA LUNT

CHASE, A. F. and RICE, P. M. (eds.). *The Lowland Maya Postclassic*. Austin, University of Texas Press, 1985. vii + 352 pp., illus. \$27.50.

The Postclassic period of Maya prehistory is an obscure one. It has been believed that following the general collapse of the Maya civilisation in the central and southern lowlands at the end of the Classic period, c. AD 900, these lowland sites were abandoned. Many of the inhabitants are then thought to have migrated north to the Yucatan where a modified form of Maya civilisation briefly flourished. The modification is considered a result of foreign influence and invasion by groups such as the Toltecs and Itzan. Their influence enabled a social revival at sites like Uxmal, Tulum and Chichen Itza. This interpretation, however, is based on rather limited archaeological observation. Compared to Classic period study investigation has been meagre. The Postclassic period has not received the archaeological attention required to know and understand accurately what happened during this period, c. AD 900-1500. I am pleased to say that this volume helps to redress this situation.

The book consists of a collection of 19 essays - 10 from the AAA meeting of 1979 - as examples of the latest state of the art study on the Maya Postclassic. The papers are separated into 3 geographical sections: the Northern Lowlands (Yucatan and northern Belize), the Southern Lowlands (the Peten), and the Peripheries (regions to the west and south). The editors provide an introduction to each section and round off the text with an introduction and conclusion. There are no specific methodological or theoretical themes, simply a presentation of new research. For this reason the editors' arrangement of papers on a geographical basis is a sensible one.

A few thematic questions are addressed by many authors in their essays though. These are:

1. Was there cultural continuity or discontinuity from the Classic to the Postclassic, as well as during the Postclassic, in the respective sites or regions studied?
2. Is there evidence for external influence on the region?
3. Is there cultural homogeneity or heterogeneity between adjacent regions?

These three interrelated questions are addressed in order to assess the extent of the Classic collapse and the subsequent foreign influence, and to monitor Postclassic change and development. The evidence generally reveals that there was some continuity from the Classic period, there was evidence for foreign influence and there was evidence of regional change and development. There was both cultural homogeneity and heterogeneity between adjacent regions, the southern lowlands were not abandoned to the extent previously believed, and in the peripheral regions the data suggest that the Maya were influencing surrounding cultures as much as these cultures influenced them. In other words, the evidence reveals that the Postclassic lowland Maya continued to survive as a vibrant interacting society. This, to say the least, presents a rather different picture of the lowland Maya Postclassic than the one previously imagined.

This is an important text. For too long too little information has been available on this period. Scholars who attempted to interpret many social, religious, political and economic customs and institutions of the Classic period used native and Spanish ethnohistoric literature to aid their interpretations. In some instances customs remained startlingly similar, eg. sacrifice and ancestor worship, while others had completely changed or even disappeared, eg. political organisation and certain agricultural practices. The study of the Postclassic period will help us to understand how and why some changes occurred and others did not. This volume is only a beginning but it does get us on that road. For that reason this book is essential reading for every scholar and student of the ancient Maya.

BRUCE WELSH

MINNIS, P. E. *Social adaptation to food stress* (Prehistoric Archaeology and Ecology series). Chicago, University of Chicago Press, 1985. x + 239 pp., 33 figs. £18.50.

This book, which must be described as one of the 'New Archaeology', is refreshingly devoid of jargon and tortuous, polysyllabic construction. The style is straightforward, the arguments are presented in a clear and well reasoned sequence, and the author is never afraid to admit when the evidence is poor or the reasoning weak. This is an excellent example of scientific method: the aims of the study are clearly described, background information is presented, the analyses are preceded by discussion of their application and followed by concise summaries, and the conclusions drawn are self-critical and lucid.

The book may be divided into three sections. The first comprises Chapters 1 and 2, and provides an introduction to the study, citing previous work, ethnographic parallels and describes the model of food stress to be used for the study. This is based upon ecological models, so has a sound basis, and logical modifications are made to fit the prehistoric, non-colonial human situation. Essentially the 'model is that with increasing stress severity, there should be increasing social inclusivity of the responses used' (p.23). Minnis defines four levels of response: household, kin group, community and extracommunity, and concludes that each level will require greater cultural change which should be recognisable in the archaeological record.

The second section, Chapters 3 and 4, consists of a detailed summary of the archaeology and natural environment of the region of study, the Rio Mimbres region in south-west New Mexico. A number of sites were surveyed and excavated, providing a sample of all the main environmental zones within the region. In Chapter 4 an attempt is made to reconstruct the past climate of the region, mainly relying upon dendrochronological work in an elegant and simple study, and also using evidence from charcoal and documented evidence.

Chapters 5 and 6 form the main part of the book and the heart of the study: estimation of food stress occurrences and the responses to them. The subsistence economy is

reconstructed using evidence from animal and plant remains (the emphasis on the latter perhaps reflecting the author's specialisation), and from coprolites: maize is shown to be the staple food. Subsistence failure is considered, with land and water availability discussed in relation to the previous reconstruction of climatic fluctuations to provide estimates of periods of crop success and failure. Stress is then calculated on the basis of length and severity of crop failures, with periods of stress being defined in four categories. The naturally available foods are considered in a detailed series of calculations (some rather tenuous), and it is shown that during drought conditions there would have been insufficient natural foods to supplement crop failure for some of the periods considered. Responses to food stress are assessed in terms of exchange systems, with these being divided into extra-, inter- and intra-regional exchange. The data were insufficiently precise to evaluate the last of these, so only the first two categories are assessed. Degree of social integration is also considered, and Minnis concludes that certain periods of stress can be fairly unequivocally defined, whilst others are more imprecise.

The book is rounded off with a concluding chapter which discusses the study in wider terms, noting the limitations for archaeological situations. A true interdisciplinary study, drawing upon sources as varied as cultural archaeology, ecology and climatic reconstruction, this is an excellent model of the use of archaeological and environmental studies to provide an insight into environmental fluctuations and the responses of human communities to them.

BRUCE LEVITAN

LOWE, John W. G. *The dynamics of apocalypse: a systems simulation of the Classic Maya collapse*. Albuquerque, University of New Mexico Press, 1985. vii + 275 pp., 25 figs., 4 tables. \$22.50.

Despite the purple cover and the equally lurid title this is a scholarly and rewarding book. It is also eminently readable in the way the J. E. S. Thompson's books are. To my mind it is probably the most exciting book to emerge from the Mesoamerican scene since Kent Flannery's *The Early Mesoamerican Village*.

Lowe adopts a Systems Analysis approach to the Maya collapse, ie. he seeks to define the elements important in a system, assesses the development of the relationships between the elements, and investigates their dynamic consequences. The resultant model is a selective synthesis of pre-existing models and new ideas so that a new approach is the end result. This is not in any way, though, an account of obscure statistical practices. Lowe provides a glossary of terms, programs, data used, and defines anything open to ambiguity. The discussion of the potential use, value and reliability of the various categories of data available for model simulation is clearly (and honestly) presented. Lowe pulls no punches in assessing the validity of categories of data and comments on the suitability of certain approaches.

Chapter 3, 'Simple Causal Models of the Maya Collapse', briefly investigates the current theories relating to the environmental/ecological, ideological, technical and socio-political causes posited for the collapse. A *tour de force* in its own right, this is a masterly and stimulating synopsis of all the current ideas concerning the collapse and a criticism of their various elements. This chapter alone is of use to the student; it provides a far more intelligible version of many of those theories than the original authors did.

The succeeding chapter, 'Systems Models', gives a very clear explanation of various models of social dynamics and their applicability to the Mayan collapse. It contains a useful exposé of the pitfalls encountered in assessing the value and dependability of the factors that may be involved in any of the models under discussion.

Less satisfactory is the chapter dealing

with historical parallels, which takes Mesopotamia and Greece as comparisons for the Mayan situation. It seems unwise to draw too close a comparison between one set of incomplete data and another, as archaeological data must inevitably be.

The abstract principles behind the statistics used proved too much for me - as I suspect it may for the majority of readers, but that does not detract from the general interest of the book.

And now to the final conclusion as to why the collapse happened. Lowe suggests that the pattern of the collapse is not random. It moved from the outside in, and broadly, from the SW to the NE, not as a wave, but as a definable tendency. The collapse was not, therefore, coeval and cataclysmic. There was a gradual movement towards a system where states moved into a marginally unstable condition, which, on the basis of monument dates seems to have been around AD 750. External pressures gradually exerted an irresistible force driving towards an inevitable collapse in a finite amount of time. The collapse is ultimately attributed not to ecological factors but to administrative failures.

The compelling quality of Lowe's argument resides in a number of factors. Firstly, the mathematical modelling and the archaeological evidence chosen do seem compatible. Thus it makes sense that the Northern (Puuc) sites continued their existence apparently unscathed by the disasters happening away to the south. The distance between the sites concerned would account for this. The occasional elements that are clearly 'southern' which crop up in the north are suggested as being the result of small population influxes. Lowe isolates Northern and Central Belize as areas which were relatively untouched by the collapse. The explanation offered in this case is that Belize had a dense population, and agricultural intensification but lacked a vulnerable socio-political and redistributive apparatus. The presence of the geomorphological barrier (the Maya Mountains) isolated it from the Central Peten. Similarly, the SW origins of the collapse were also rather remote from this eastern area.

The general quality of the book made the minor slip-ups both prominent and irritating: 'Kulcalcan' (p.65) is a mis-spelling

of Kukulcan ('Kukul': feather, 'can': snake), the Maya dialect name for Quezalcoatl ('Plumed serpent'). In turn, this is confused with Topiltzin Quezalcoatl (the man). Similarly, the Motagua River is mis-spelt as 'Montagua'. These are minor points in an otherwise rewarding book. It could easily have been a self-conscious and loose aggregate of half-facts and jargon. The success in communication rests in the clarity of the language used and the fact that Lowe knows what he is talking about. Despite the minor quibbles cited it is clearly a well-researched work, and worth any investment the student or scholar might make.

LEA D. JONES

PLOG, Stephen (ed.). *Spatial organization and exchange: archaeological survey on Northern Black Mesa* (Publications in archaeology, Southern Illinois University at Carbondale Center for Archaeological Investigations). Carbondale and Edwardsville, Southern Illinois University Press, 1986. xx + 377 pp., illus., tables. \$30.00.

In this volume Stephen Plog and his five collaborators, Jeffrey Hantman, Elizabeth Garrett, Margerie Green, Mark Cattin and Michelle Hegmon, present the latest results of the Black Mesa Archaeological Project, a cultural resource management programme in North East Arizona that started in 1967 and is still running. As a result of the Peabody Coal Company's decision to strip-mine a 120 sq km area (the Eastern Lease), the opportunity arose to make an intensive, systematic survey of an area with known archaeological potential but without any previous long-term investigations. The result is a volume which challenges European archaeologists to match the Black Mesa project for theoretical elegance, methodological rigour and definition of soluble problems. This volume is one of the most impressive monuments to the research directions established two decades ago by the members of the South Western Archaeological Research Group.

The cultural sequence at Black Mesa covers two and a half millennia, from c.1300 BC to AD 1150. In the survey a total of 769 sites, including 820 components, was discovered. In the majority of cases the sites fell within the last three centuries of the sequence and this period consequently forms the main focus of analysis. In these 300 years three major cultural changes occur - the transition to food production, a long-term if fluctuating rise in farming populations and the virtual abandonment of the area between AD 1075 and 1150. The principal theoretical problematic is the explanation for these major changes.

Before evaluation of the theoretical project we should turn to the Black Mesa data base. The contributors ground their approaches in the regional scale of investigation, in which the results of site excavation are used to improve understanding of the region as a whole and vice versa. There are, however, two additional advantages for this project which enhance the dialectical relationship between site and region. First, the quantity of prior excavation provides a solid data base which is creatively used to help explain surface artifact patterning. Secondly, the sophistication of the regional chronology, based on regression analyses of tree-ring dates and associated painted wares, is such that phase divisions are based on as short a period as 25 years. Such tight chronological controls will long be the envy of all European prehistorians and not a few Romanists and Medievalists. This chronology provides the basis for the most detailed analysis of settlement and population history and exchange ever attempted in this region. In this way it is also possible to clarify the relationships, causal or otherwise, between most of the relevant cultural and environmental variables.

As regards the origins of food production on Black Mesa (in South Western terms, the Basketmaker-Pueblo transition), a cluster of causative social, technological and environmental changes is identified. The principal problem is that much of the Southwest, and certainly Black Mesa, was marginal to agriculture. An environmental shift towards low regional variation in precipitation is interpreted as permitting a more predictable crop yield, which in turn favoured increased reliance on cultigens. The buffering mechanisms against crop failures include storage (pottery and rooms) and exchange, both centred on public ritual buildings, or kivas,

constructed on increasingly permanent settlements. However, it appears that all these developments followed the spread of food production. Plog promises further excavations at sites straddling this critical transition and emphasises the need for further studies on the productivity of Southwestern cultigens. The origins of agriculture are not yet clear in this area; this is one of the 'Big Questions' of prehistory to which archaeological survey has so far made little contribution.

The adoption of farming and sedentism is used as a platform for the explanation of the long-term growth in population on Black Mesa. The biological and economic factors included in Plog's model rely heavily on Binford and Chasko's Nunamut demographic studies and represent a major advance over simplistic ideas of population growth as an independent variable. Before assessing the models used to account for this pattern, it is valuable to consider the methods used for estimating Black Mesa populations.

A critique of past techniques of using counts of rooms per site is based on objections that different rooms have different functions and that residential areas do not necessarily leave above-ground traces. Instead, a statistically significant correlation is found between the minimum number of dwellings on excavated sites and the product of site area and surface artifact density on those same sites. The result is used as a means to calculate population size for unexcavated sites. Such a technique ignores the variety of possible relationships between surface and sub-surface deposits, an omission which may account for much of the substantial residual variation left unexplained by the multiple regression curve. Site population data is then combined using two estimates of occupation duration (15 or 25 years), itself based on tree-ring and ceramic dating, to produce population trends over the period AD 850 to 1150. There are two main results: first, a long-term population growth, from c.300 to 1400 people; and secondly, the proposal that, in the period of highest growth rate (AD 1051-1075) over 70% of the increase was produced by immigration.

The fluctuating population levels on Black Mesa is explained by F. Plog in terms of a cyclic pattern of resilient and stable economic strategies. Resilient strategies are used by stable populations of more egalitarian groups in small farmsteads cultivating local resources and having few trade goods.

By contrast, stable strategies were employed by growing populations of ranked societies whose agricultural intensification enabled wider inter-elite trade networks and whose territorial integrity was founded on close ties with central places. A general correlation between the quantity of storage and the size of populations is cited as evidence for this model, despite the objection that societies who required more storage could presumably have constructed it. As with Halstead and O'Shea's model of social storage in the Aegean, there is a tendency for a normative idealist bias in such a model, which excludes any discussion of exploitative relationships.

In discussing the abandonment of Black Mesa, Plog happily rejects theories relying on environmental determinism or carrying capacity assumptions, not least because population decline began long before any climatic deterioration. Instead, Plog favours the theory of systematic hyper-coherence, in which groups are so well adapted to the local environment that their capacity to buffer radical environmental change is reduced. One facet of this model is the adoption of agriculture in neighbouring areas, which removed the potential for buffering exchange systems between foragers and farmers.

The explanations for the three major changes in Black Mesa prehistory are based on a methodology redolent of the golden years of the New Archaeology. The research method is nothing if not problem-oriented and a barrage of statistical techniques helps the reader to understand the relationships between key variables. Whilst the stats are usually impressive, there is an unfortunate tendency to dichotomize continuous variables and attempt to define meaning through the use of significance tests. Indeed, since 262 significance tests are presented *in toto*, 26 of them would indicate significant relationships by chance alone if a 0.1 level of probability is used. Given the range of available multi-dimensional techniques, the over-reliance on significance tests seems a trifle outmoded.

If there are weaknesses in this valuable volume, they lie in the realms of theory. Explanations of cultural change have clearly progressed from the early days of New Archaeology, but mainly through the evolution of more complex models of economy-environment interactions, with buffering

mechanisms representing the 'social' factors. Although important concepts (breeding networks, alliances, exchange networks, etc.) are included, their effects on social action and the creation of power relationships are rarely considered. There remains much potential in modelling the social causes of shifts from resilient to stable strategies and vice versa, as there is in operationalising models of alliance structure.

Perhaps the development of a body of genuinely social theory will be the next challenge for Southwestern archaeology. There can be little doubt that the data base in archaeology's 'Garden of Eden' is one of the best available with which to evaluate theories of social power. One can but congratulate Stephen Plog and his associates for their contribution to this state of affairs.

JOHN CHAPMAN

HILLSON, Simon. *Teeth* (Cambridge Manuals in Archaeology). Cambridge, Cambridge University Press, 1986. xix + 376 pp., 110 figs., 18 pls., 38 tables. £27.50.

Teeth play an important role in most peoples' lives, as we are all too often so painfully reminded by frequent visits to the dentist. But few would ever realise just how much information is locked within the complex calcified tissues that constitute both human and animal teeth.

Simon Hillson's book explores the various avenues of dental research and shows the vast potential of studying teeth from archaeological sites.

The first major section deals with general mammalian teeth morphology, form and function and its variation between the major mammalian orders. This section is most useful as a general identification guide to mammalian dentition during preliminary post excavation sorting of bone assemblages. The axonometric illustrations, with detailed and systematic descriptions of each tooth row, provides a

useful broad basis on which general sorting and tentative identifications can be attempted. They provide an intermediate and supplementary step between the archaeological tooth row and the comparative specimen.

Some of the illustrations are a little simplified and on some pages, where more than, say, six appear on one page, they may cause slight visual confusion, appearing crowded and possibly too small. The major headings, being at a similar angle to these tooth row illustrations, may serve to crowd the page even more. However, additional illustrations showing sections through various teeth and occlusal surfaces provide further information to aid separation of morphologically similar teeth.

Dental histology is dealt with in this book in a systematic and comprehensive fashion. Understanding the rather complex nature of dental tissues can be a rather daunting prospect to those not familiar with dental terminology. However, descriptions of the various dental tissues and the processes involved in their subsequent formation are explained in a straightforward manner and should be readily comprehensible to the uninitiated, while providing sufficiently detailed information for students of dentistry.

Ageing, metrical variation and oral pathological conditions are also dealt with in a detailed manner, using mainly human examples, and show where the wide range of the author's expertise lies. Examples of animal studies are less frequent, reflecting where the emphasis of modern and archaeological dental research has occurred. The potential for studying archaeological animal teeth in similar detail to those human studies already undertaken is, however, not quite as fully expanded on as the subject deserves.

The bibliography is extensive and contains a wide range of dental literature which will satisfy even the most fastidious tooth buff who now has ready access to many more of those obscure references.

Teeth will at the very least amaze you merely with the incredible amount and diversity of information which dental studies can provide. A practical and interesting book for the mildly interested and specialist alike.

K. M. DOBNEY

FIELLER, N. R. J., GILBERTSON, D. D. and RALPH, N. G. A. (eds.). *Palaeoenvironmental investigations: research design, methods and data analysis* (Symposium of the Association for Environmental Archaeology 5a) (BAR Int. Ser. 258). Oxford, British Archaeological Reports, 1985. 289 pp., illus. £20.00.

FIELLER, N. R. J., GILBERTSON, D. D. and RALPH, N. G. A. (eds.). *Palaeobiological investigations: research design, methods and data analysis* (Symposium of the Association for Environmental Archaeology 5b) (BAR Int. Ser. 266). Oxford, British Archaeological Reports, 1985. 254 pp., illus. £20.00.

These two volumes are the result of an Association for Environmental Archaeology conference held in Sheffield in 1983. They represent the fifth of the Association's conferences to be published by BAR. The two volumes have different titles: *Palaeoenvironmental Investigations* contains work on sediments, soils, statistical methods and past environments (including geomorphology); *Palaeobiological Investigations* contains work on vertebrates, insects, molluscs and botanical aspects. However, the two volumes must be considered together because they have identical subtitles and editorial introductions which emphasise the importance of research design methods and data analysis in the development of conceptual models in environmental archaeology. This point emerges from some of the papers but apart from that they do not have a distinct theme and are really collections of essays. This must, I think, be regarded as a weakness at a time when environmental archaeologists are being challenged (as Professor Coles challenged us at a subsequent Norwich meeting) to demonstrate our objectives, clearness of purpose and political relevance. These volumes contain little which helps us to meet this challenge; in many papers the emphasis is on data collection rather than aims. Even so they describe a number of promising advances in the boundary areas between traditional academic disciplines, particularly the geoarchaeological and related aspects for which Sheffield is establishing a justifiably good reputation. The high spots of these volumes include several useful papers on taphonomy (including Briggs *et al.*, Stallibrass, Payne and Munson) and some valuable discussions of analytical techniques which are relatively

new to archaeology, such as mineral magnetic properties (Oldfield *et al.*), micromorphology (Fisher and Macphail) and studies of molluscan seasonality (Deith). I particularly enjoyed a paper by Mills on geomorphological factors affecting settlement survey in South East France and a thought-provoking discussion by Thomas of the problems of interpreting sub-fossil landsnail assemblages. Some of the other papers represent progress reports on research which has not yet reached the stage of definitive conclusions.

It will be obvious from the foregoing outline that the content of these two volumes is extremely varied, both in terms of subject matter and, to some extent, relevance to archaeology. At over 250 pages each the volumes represent a mammoth production and we must be grateful to the editors for their considerable work and for the many good and useful papers included (some of which were not given at the original conference). At the same time I for one regret the absence of a clear theme or message which would have helped to ensure that the value of the volumes was more than the sum of the constituent parts.

MARTIN BELL

NELSON, Ben A. (ed.). *Decoding prehistoric ceramics* (Publications in archaeology). Carbondale and Edwardsville, Southern Illinois University Press, 1985. xiv + 441 pp. \$35.00.

There is something of interest for most students involved with ceramics. The diversity and detail of the papers makes it impossible to give an impression of them all, but the themes are divided into four sections: Stylistic Variation and Social Organisation; Organisation of Ceramic Production; Assignment of Form, Function and Context; and Further Lessons from Ethnoarchaeology. A fifth and final section is 'Comment', which is less of the traditional synthesis expectable in composite works of this sort than a

discussion of the evolution and change of ceramic style; in short, a separate contribution in its own right.

For those interested in the application of mathematical methods and models to ceramic studies, there are several papers involving these techniques, eg. 'Use of Multidimensional Scaling to Display Sensitivity and Symmetry Analysis of Patterned Design to Spatial and Chronological Change: Examples from Anasazi Prehistory' (R. G. Matson and D. K. Washburn); 'Changes in Ceramic Production in Pre-Hispanic Oaxaca, Mexico' (G. M. Feinman); and 'Reconstructing Ceramic Vessels and the Systematic Contexts' (B. A. Nelson).

One of the best papers from a practical 'dirt archaeology' stand-point is that by Barbara Stark: 'Archaeological Identification of Pottery Production Locations: Ethnoarchaeological and Archaeological Data in Mesoamerica'. This focuses on ceramic waste material (pre- and post-firing), raw materials, structures associated with firing (kilns and hearths) and the tools of the potter's trade. There is undoubtedly a need to draw attention to the less obvious ceramic-associated features in order for them to be recognised, perhaps in contexts where they may not be automatically expected. This data helps plug the gap all too commonly seen in ceramic studies; ie a close focus on trade patterns to the detriment of the more parochial aspects of ceramic production.

Despite the apparent breadth of subject matter represented in the book, it would have benefited from a more hard-core inclusion of technological studies. Without this, the title is misleading.

The use of the statistical methods presented here should be viewed with healthy caution; other sets of data may be less suitable for such applications. Nevertheless, as a reference work it will be of interest to a wider range of students than many works of a similar nature.

LEA D. JONES

ARNOLD, Dean E. *Ceramic theory and cultural process* (New Studies in Archaeology). Cambridge, Cambridge University Press, 1985. xi + 268 pp., illus. £19.50.

The book provides a synthesis of most aspects of pottery manufacture and subsequent dispersal. Arnold concentrates on current anthropological observations of practice and technique and deliberately avoids involvement with typologies.

Some of the information is interesting and stimulating. For example, the effects of climate on pottery manufacture, where wetness and low temperatures condition the organisation of the potter's year, there are attendant social and economic repercussions. Similarly, a section entitled 'Scheduling Conflicts' presents a good insight into how potting is accommodated with other, daily or seasonal, tasks. The role of men and women in pottery manufacture within these 'schedules' is also considered. The section dealing with 'Sedentariness' looks at the lifestyle of certain groups such as the nomads or semi-sedentary peoples and their relationship with ceramic technology.

The superior technological advantages of clay vessels is emphasised. A large number of common vegetable foodstuffs appear terrifyingly dangerous unless cooked - and ceramic vessels provide the best, all-round cooking devices.

The strengths of this book lie in the pooling of anthropological data relating to pottery manufacture, and the angle afforded in the presentation of the data. Unfortunately, there are many weaknesses. Firstly, the attempt to skew this information into archaeological contexts is unsatisfactory:

'The thesis of this book is that there are certain universal processes involving ceramics that are tied to ecological, cultural or chemical factors. These processes occur in societies around the world and can provide a solid empirical ... base for interpreting ancient ceramics' (pp.ix-x).

The attempt to marry current anthropological practices to ancient evidence is always fraught with danger, but in this case it has encouraged the author to dismiss certain

archaeological data on the grounds that they do not conform to predictive models constructed from modern information. Arnold demonstrates this on p.59 in relation to volcanic ash importation into the Maya Lowlands; most archaeologists would do well to choose their own applications for this type of information carefully and to be circumspect about the implications for archaeology as offered here. Indeed, I remain unconvinced that the archaeological applications were the *primum mobile* of this work; the concluding chapter contains the briefest of arguments for the role of these data in archaeology.

A more general criticism concerns the style of writing, which could easily constitute a barrier to communication. As an example:

'Thus the book attempts to explain why these regularities successfully retrodict evolution of ceramic specialisation in the past' (p.x).

There are many similar examples to be found. The introductory chapter is particularly impenetrable. This is not only due to the excessive use of jargon, but to Arnold's use of the language and concepts of other disciplines, without it seems, a proper understanding of either.

The book does have interesting and useful information to offer, but it will probably find a more comfortable niche in anthropology reading lists, rather than those dealing with archaeological ceramics - the topics related to, and problems posed by them.

LEA D. JONES

KLEJN, Leo S. *Archaeological typology* (translated from the Russian by Penelope Dole) (BAR Int. Ser. 153). Oxford, British Archaeological Reports, 1982. 321 pp., 25 figs. £13.00.

This book is both an historical account

of the role of, and opinions about, typology in archaeology, and an exposition of the author's views of the subject. The first two parts (about half of the book) take us through the history of 'types' and typology, from Montelius to modern writers in Russia, the USA and Britain. A contrast is drawn between 'empirical types', derived from the formal study of material, and 'cultural types', which have a meaning of some sort within their own historical context. The debate, sometimes a bitter one, between those with opposing views (eg. Taylor vs. Brew, Spaulding vs. Ford) is followed through, and the weaknesses of those views are exposed. One is left with the feeling that none of the stated theoretical positions is satisfactory, and clearly this is the author's view, as he says 'it is only possible to identify cultural types from among empirical types if we base ourselves on archaeological cultures' (p.141), an apparent reversal of usual archaeological practice.

The account seemed, at this stage, to have two main weaknesses: (i) although dealing partly with mathematical concepts, and accepting the relevance of mathematics, the author does not use mathematical language where it would be appropriate, and indeed desirable, to clarify certain points; (ii) the author seems to give too much weight to what archaeologists say, rather than what they actually do. Archaeologists are often better at doing archaeology than at creating a theoretical framework to explain what they have done.

Having lifted the debate to the level of cultures, the author has a long section (Part 3) on 'what is an archaeological culture?'. Again, we have a history of the theory, and the typological question is raised to a higher level by posing the question, are archaeological cultures 'real' or simply convenient constructs? After devoting a chapter to the question of the relationship between 'culture' and 'ethnos', and looking at other questions such as the contrast between 'type' and 'style' (p.190 ff), and the use of inductive or deductive argument (p.223), the author brings us full circle to archaeological cultures as a starting point for the study of types.

After the previous section, this one felt rather repetitive. The author seems willing to create tidy logical structures and introduce new terms to fill spaces in them which might better be left blank. In

pp.197-202 there are at least twenty new terms for various levels of association and aggregation of types, for example, concern-stencil-coherent-ensemble-booklet-period. New terms are needed for new concepts, but on this scale the outcome can only be confusion. Little use is made of this terminology in the rest of the book.

Finally, the last 30-40 pages give us the author's own views. He calls for a new strategy, which he calls 'system strategy', to replace the inductive and deductive strategies which have been tried and found wanting. His strategy is a 'top-down' approach, proceeding from culture to cultural types to culturally significant attributes, in contrast to the conventional 'bottom-up' or 'grouping' approach. He defines types as 'stable schemes showing how the appearance of the artefacts corresponds to their position in characteristic cultural contexts'. The ground rules of the strategy are provided by four 'principles of impossibility' (pp. 266-68, cf. Heisenberg's Uncertainty Principle), and the launching platform is intuition. He points out that even the most analytical work starts from intuition in the exposure and selection of attributes. Before explaining his approach in detail, the author points out that the implementation of such a strategy is not new - many archaeologists have worked in the ways he suggests - but that what is new is the theoretical basis, which he sees as a light to the feet of the stumbling practician (p.286).

It is difficult to do justice to the proposed procedure (pp.276-84) in a summary. The first stage, '*a priori* exposure of cultures', is roughly speaking the recognition of the spatial and stratigraphic patterning inherent in one's material, including the recognition of 'evident' types and the old idea of 'type-fossils'. Style is seen as an important factor, which cannot be formally defined but which possesses a ready identifiability. Next comes the deduction of types from cultures; simplest are the evident empirical types, but the rest must be teased out by means of a 'structuro-contextual plan' of the culture, which generalises the remains and traces of different types of activities, taking into account theory, ethnographic parallels, archaeological common sense and comparison with other, related, cultures. Finally one has to look at the diffused material, where there are no clear-cut boundaries and the empirical types are indistinct.

This section is unconvincing without a worked example based on real material, preferably a reworking of an existing typological system. The author acknowledges this point, and explains why it could not be done. As he implies, the test of his approach is whether it is taken up successfully by a fresh generation of archaeologists, and he allows a space of 20 years for this to happen (or not). Unless there is an early convincing application of the 'system strategy' to show the way and encourage others, its acceptance is unlikely. Nevertheless, the book makes some good points, eg. 'the traditional under-estimation of the theoretical implications of the initial processing of material' (p.89), and would be food for thought for anyone about to embark on a typological exercise.

The translation is good and there are few obvious problems with technical terms - 'goblet' surely means 'beaker' (p.57 and elsewhere), while 'morphology' might be better than 'topology' (p.77). The usual BAR standard of litho-printed typescript does not make the book easy to read, and there is a serious production error (pp.215-16 repeated in place of pp.211-12) in my copy at least.

CLIVE ORTON

MACREADY, Sarah and THOMPSON, F. H. (eds.). *Archaeological Field Survey in Britain and Abroad* (Occasional Paper [New Series] VI, Society of Antiquaries). London, Society of Antiquaries of London, 1985. 251 pp., 80 figs. £15.00.

Britain has always had a very strong tradition of archaeological fieldwork involving techniques other than excavation. The Royal Commission on Historical Monuments' surveys remain models of their kind. Individuals like O. G. S. Crawford in the 1930s and P. J. Fowler in the 1960s stand out as trailblazers in the art of non-destructive field archaeology. With an expansion of resources in the 1970s, together with a

fundamental shift away from the concept of sites separated by blank areas, numerous extensive surveys were undertaken, both in Britain and abroad. These surveys have revolutionised how archaeologists now think of man in the landscape.

This volume is essentially a series of case studies from Great Britain, France, Spain, Yugoslavia, Greece and North Africa. The unifying theme is the study of regions or landscapes rather than sites. Naturally, with regions as diverse as the Lincolnshire Fens and Dalmatia, approaches and methods will vary.

Section 1 consists of a general overview of field survey in Britain by Roger Mercer followed by four case studies from Eastern England, Lincolnshire, Cornwall and Berkshire. These neatly contrast surveys of areas with visible structures, like Cornwall, and areas where the archaeology survives largely as scatters of artefacts in various densities on the landscape.

The division between the basic methodologies required in a highland zone and a lowland area is reflected in all later sections. Section 2 considers three surveys in France, all involving detailed surface collections of artefacts. The paper by R. F. J. Jones *et al.* on survey in Catalunya in the section on Spain is particularly important in its consideration of exactly what an archaeological survey can achieve given that landscapes are not static and that archaeological data are always being eroded, moved, changed, buried, exposed and reburied again.

Throughout the first 224 pages of this book there is little doubt as to why archaeologists are expending time, labour and money on field survey. It is to advance archaeological knowledge. Section 5 on North Africa brings archaeologists rapidly down from their ivory towers. 'If archaeology is to be practised at all, then at least let it be relevant to the needs of people today.' So Colonel Ghaddafi initiated a survey of Ancient Agriculture on the Saharan Fringe. The resulting study of Roman agriculture in Libya is a model of both archaeological fieldwork and relevance of archaeology to people today.

This volume is extremely well produced with eighty clearly reproduced line drawings and eight plates. At £15.00 it is very good value and must be considered essential reading for all students of archaeology, parti-

cularly anyone contemplating undertaking a field survey.

P. L. DREWETT

HIETALA, Harold J. (ed.). *Intrasite spatial analysis in archaeology* (New Directions in Archaeology). Cambridge, Cambridge University Press, 1984. vi + 284 pp., illus. £30.00.

This book consists of 12 examples of work on the spatial aspects of archaeological sites from the early Pleistocene to 20th century ethnographic examples, and including village, pueblo, metropolis and fortress sites as well as the expected hunter/gatherer sites. The editor has added a brief introduction and some thoughts on future directions.

Three related themes emerge: (i) the need to develop methods of analysis based on archaeological models, following a period when many techniques were simply borrowed from other disciplines; (ii) the need to ensure congruency between the questions being asked and the techniques used to answer them; and (iii) the need to look at problems on an appropriate scale (expressed as a contrast between 'global' and 'local' methods). Each author tackles one or more of these problems, as well as looking at more specific topics.

Of the seven methodological papers, three present a 'new' approach (Chapter 5, Berry, Kvamme and Mielke's multi-response permutation procedure [MRPP]; Chapter 6, Johnson's local density analysis; Chapter 13, Whallon's unconstrained clustering), while four develop the use of an existing statistical technique (Chapter 4, Hietala, log-linear models; Chapter 9, Ciolek-Torrello, factor analysis, principal components analysis and multi-dimensional scaling; Chapter 10, Cowgill *et al.*, cluster analysis and discriminant analysis; Chapter 12, Spurling and Hayden, Fisher's exact test and Dacey's mean splitting procedure).

MRPP, a conditional technique based on fixed data locations and variable data types, seems ideal for some sorts of spatial analysis, eg. of cemeteries. The example chosen does not show off the technique to its advantage - surely a simpler measure of association than one based on all inter-point distances could have been found, and the use of circular quantiles as measures of dispersion seems unnecessarily restrictive when elliptical ones would have been nearly as easy and much more general. The mathematics in this paper may deter some potential users, but the approach is worth careful consideration.

Although 'new' in the sense of developed specifically for archaeology, local density analysis has been around for some time (first published by Johnson, 1976). Graham (1980, 110) pointed out that in some cases a spatial distribution could in this approach appear to be more similar to another than to itself, but no notice seems to have been taken of this criticism. The technique can now be seen as a special case of a K-function (Ripley, 1976).

Unconstrained clustering is the most original of the techniques. Whallon's approach, which in brief consists of (i) smoothing the spatial distributions of artefact types, (ii) classifying locations according to the relative proportions of the smoothed values, (iii) using cluster analysis to group points into 'clusters' as a basis for interpretation, succeeds in removing many of the constraints for which other techniques are rightly criticised, eg. on the size, shape or number of clusters. The author admits to one serious problem, that of 'overlapping clusters'; given two overlapping distributions of single artefact types, the method will discover at least three clusters, including a 'mixed' one, which must be argued away in the interpretation. A second drawback is that smoothing the data actually spreads the distributions out into surrounding areas, creating a sort of 'halo' effect, so that two completely distinct clusters would give rise to a 'mixed' cluster in the empty space between them. This effect is strongly apparent in the worked example, where clusters supposedly containing an average of up to 25% of a particular artefact type do not have a single artefact of that type lying within their boundary. Although there are only five artefact types, two of which have very simple spatial distributions, 13 clusters are created. Their 'remarkable'

homogeneity reflects the simplicity of the distributions. The interpretation of the site appears to owe more to the undoubtedly skill of the author than to the usefulness of the technique.

The two 'urban' papers (Chapters 9 and 10, Ciolek-Torrello on pueblos and Cargill *et al.* on Teotihuacan) give interesting applications of classificatory techniques in a spatial setting. They are especially valuable for the comparison of different techniques applied to the same data, and would be a useful starting point for anyone studying (eg.) the distribution of pottery across a town. Fletcher's approach to a medieval fort (Taskun Kale, Chapter 11) is frustrating. If valid, it has tremendous implications for the study of urban and military sites, but because he assumes it is, and straightjackets his data into it, it is impossible to tell whether or not he is justified. A critical reworking of the data would be valuable.

Many useful archaeological points are made in the other papers. For example, Kroll and Isaac (Chapter 2) discuss how patterning arises on early Pleistocene sites, and Hodder and Hivernel (Chapter 7) examine the blurring effects of post-depositional factors. A notable point throughout the book is the way in which archaeological reality tempers statistical enthusiasm.

Overall, I found the archaeological aspects of this book interesting and the statistical ones rather disappointing. While it is right to cease to rely on techniques developed for (eg.) plant ecology, and to seek ones more appropriate to archaeology, it seems unwise to ignore the theoretical work being done by statisticians like Ripley (one very general reference in Chapter 14), Besag or Diggle or the Spatial Data Project at the University of Bath (no references). Independence is good, insularity is not.

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CLIVE ORTON

LANGLEY, Susan B. and UNGER, Richard W.
Nautical archaeology: progress and public responsibility (BAR Int. Ser. 220). Oxford, British Archaeological Reports, 1984. 214 pp., illus. £10.00.

This report incorporates papers and discussions of internationally respected archaeologists who took part in a 1983 conference in British Columbia, organised by the Archaeological Institute of America and the Vancouver Maritime Museum.

The organisers felt that while worldwide interest in underwater archaeology has been growing, policies for dealing with increased exploration have been slow to appear. They hoped that by exchanging experiences the professionals, amateurs and lawmakers could learn where pitfalls lie and how best to respond to them.

The opening speaker was Margaret Rule, to whom the volume is dedicated, although due to a 'technical failure' her paper has not been included. Instead, there are a number of less familiar excavations, such as the wreck of the USS Monitor, which sunk off the coast of North Carolina after the first battle between ironclad warships during the American Civil War; and a paper on the Dutch treatment of some 350 wrecks discovered in the Zuider Zee since land reclamation began there in 1942.

The legal position of historic wrecks, world-wide, is investigated in the first paper, surveying recent legal developments relating to nautical archaeology. Whereas coastal waters enjoy a degree of control, shipwrecks on the deep seabed are at present subject only to laws of salvage, which are 'seriously dysfunctional for the preservation of the underwater cultural heritage'. (What a pity the discovery of the Titanic post-dates this paper.)

These papers propose a range of solutions to the problem of the protection of the underwater heritage: the USS Monitor, for example, was designated a marine sanctuary in 1975, and in Canada the Fathom Five Provincial Park is Ontario's only park to incorporate 72 square kilometres of lake bed.

Conservators emphasise the dangers of hasty investigation, without due regard to the preservation of artefacts; and the emphasis throughout is on the value of historic shipwrecks as an irreplaceable and non-renewable resource, deserving due care and protection.

ANNE JONES

RENFREW, Colin. *Approaches to social archaeology*. Edinburgh, Edinburgh University Press, 1984. viii + 430 pp., illus. £17.50.

This is a collection of Colin Renfrew's most significant theoretical papers published since 1972. This in itself is useful for all interested in social archaeology, but what makes the book particularly valuable is the first chapter and short introductions to each of the five sections of the book. These provide some link between the papers, and update with full bibliographic support the debates since the papers were written. For many already familiar with Renfrew's work these section introductions will be the most interesting and stimulating elements in the volume.

Chapter 1 sets out to justify social archaeology. Within an historiographic perspective Renfrew briefly explains the five themes on which he has concentrated. He also discusses the value of generalising about human behaviour, but is firmly against the search for general laws. Instead, he supports generalisations that are valid given certain conditions.

Section One is called 'Societies in Space: The Landscape of Power'. Here

Renfrew is concerned with recognising social units (which he calls polities), not just archaeological cultures. He sees power concentrated in central persons, and there are spatial correlates of this that can be archaeologically detected. Some of the assumptions can be considered simplistic - the largest centres were not necessarily the most important, as can now perhaps be seen with southern British hillforts. But here, as elsewhere, Renfrew is anxious to defend these papers as just a start, often tentative, to the study of social archaeology.

Section Two, 'Trade and Interaction', introduces the concept of the early State Module. This is an autonomous area served by a central place which, together with similar adjacent areas, forms a civilisation. Trade and exchange between these ESMs and also beyond is seen as a vital factor in social change. Renfrew is here excited by information flow, something that has since been further studied by others.

Section Three concentrates on the phenomenon of major monuments in simple societies with particular emphasis on megaliths, Polynesia and Neolithic Wessex.

Section Four is of wider interest - 'Systems Thinking: The Explanation of Continuous Change'. Renfrew has been one of the most vehement supporters of systems thinking and this could be considered the most significant part of the book. Renfrew takes a side-swipe at Marxist analyses in the section's introduction, but this would have benefited from being expanded into a rather more detailed critique. As it stands, it raises many questions and answers too few. Perhaps another paper elsewhere will be more expansive? Renfrew believes that systems thinking can accommodate internal change, and indeed is able to explain it. Some other users of the systems approach would disagree, but Renfrew, rightly in the reviewer's opinion, believes that change can come about from within a system. One of the internal causes of change that Renfrew emphasises is that of growth; as a system grows, it is likely to become more complex.

The final section, 'Discontinuity and Long Term Change', leads on from the previous one and catastrophe theory is applied to archaeology. Whilst the explanation of sudden change by gradual processes reaching a crisis point is valuable, the mathematical details seem unrelated to archaeological

data.

Colin Renfrew and Edinburgh University Press should be congratulated on the production of this fine book; it will be a valuable tool for students and research workers alike. Renfrew's most useful theoretical papers can now be found between two covers instead of spread over a dozen diverse publications. Whether one agrees or disagrees with the assumptions behind Renfrew's approach, it is now much easier to examine these and the type of social archaeology that can come from them.

HAROLD MYTUM

MCCULLAGH, C. Behan. *Justifying historical descriptions*. Cambridge, Cambridge University Press, 1984. xv + 252 pp. £25.00 (hardback); £7.95 (paperback).

This densely-written book discusses the inferences that can be drawn from historical sources and whether historical 'truth' can ever be achieved. Its main interest to an archaeological audience lies in its explicit descriptions of how conclusions are drawn from evidence (McCullagh deals on the whole with written sources as his evidence, but in many cases 'archaeological' can be substituted for 'historical' in his thesis). We may think that archaeological sources have an advantage over historical ones in that archaeologists excavate inanimate material rather than deal with written records which, in the nature of things, must reflect the bias of the writer. Yet a glance at this book will show the interpreter of archaeological evidence the pitfalls which may be fallen into even if he believes his evidence to be objective (although subject to the vagaries of deposition, preservation, retrieval, etc.).

A glance may not be sufficient, however, for this book is hard-going. Much of it seems to be repetitious at great length, and the conclusions do not really live up to the expectations. Nevertheless it will be useful

to archaeologists worried about the philosophical base of their subject. But do not be misled into thinking that it can be dipped into for useful snippets; it is a book which will take a long time to master and appreciate. It is up to individuals to decide whether their time might not have been better spent elsewhere.

HELEN CLARKE

research and experimentation in the field have greatly extended our expertise. The authors have remained throughout at the forefront of this development and have modified, expanded or rejected their techniques as described in the book. Their work has always been founded on an exemplary use of collaboration. The influence this book should have on our attitude, and guidance on our approach remain undiminished.

C. W. OLDENBOURG

MORA, Paolo, MORA, Laura and PHILPOTT, Paul.
The conservation of wallpaintings
(Butterworth series in conservation and museology). London, Butterworths, 1984.
494 pp., 154 pls., 13 col.pls. £16.00.

HAWKES, Jacquetta. *Mortimer Wheeler: adventurer in archaeology.* London, Weidenfeld & Nicholson, 1982. 388 pp., 16 pls., 8 figs. £10.95.

I believe that the most important ingredient of conservation is the approach or attitude. Coming from Germany where crafts are very thoroughly taught, I have seen that practical or technical excellence does not necessarily produce a responsible conservator. The crucial ingredient of this responsibility is that collaboration is sought when and where necessary. This is especially true today when the practitioner will find it virtually impossible to have an in depth knowledge of all the latest research, techniques and materials available.

It fell to the Italians to formulate the foundations of our modern conservation ethics (Brandi Report), and it is therefore not surprising that it was an Italian couple, Madame and Prof Mora in collaboration with Prof Philpott, who provided us with the first complete standard *oeuvre* on wallpaintings worldwide - their purpose, use, history, construction, development and (for the initiated practitioner!) their treatment. It is the fascinating lucidity with which each aspect is defined and assessed which makes this book valuable not only for all those concerned with wallpaintings, whether as an art historian, administrator or conservator, but for anybody involved in conservation.

It is inevitable that since this book was first published some ten years ago,

Although five years have passed since Jacquetta Hawkes's biography of Wheeler was published, it is appropriate that it should be reviewed in the *Bulletin* of the Institute that he founded, especially as this year sees the Golden Jubilee of that event in which he took so much pride. He might have been less proud of its new association with the College where he was both student and Secretary and which he described as 'a hypertrophied monstrosity as little like a college as a plesiosaurus is like a man'. The book provides a vivid picture of the man, and particularly of the energy and drive that typified everything he undertook. His ability to organise and his gift for leadership were developed in the First World War. This experience gave ample opportunity for resourcefulness and invention and taught him the value of discipline which he imposed upon himself in most aspects of his life and invariably expected in others.

These qualities stood him in good stead in his future career. Whether as an administrator, setting the National Museum of Wales or the Archaeological Survey of India to rights, or as the archaeologist in the field at Verulamium, Maiden Castle or Mohenjodaro, Wheeler was single minded about everything that he undertook and spared neither himself nor others. This dual role

culminated in his final task as Secretary to the British Academy, where money had to be raised, young archaeologists encouraged and a moribund organisation given new life.

The author draws much of her material from her own memory. This enhances the air of authenticity, though it is possible to detect the parts where she is not able to write from her first hand recollections, especially in the sections on the Second World War and period when Wheeler was in India. The 'Hero figure' of whom she speaks dominates throughout. Wheeler's many services to archaeology are apparent, and although some of his theories and techniques have now been discounted, perhaps his greatest achievement was that of drawing the attention of the public to its importance. This he did most effectively through his involvement in television and through Swans Tours. This 'popularising' of the discipline made him, above all else, a successful fund-raiser, and without funds archaeology cannot succeed.

The book was written too soon after Wheeler's death and from too personal an angle to become established as the definitive biography. It conveys an accurate view of his personality and achievements, his faults are not glossed over and it is both informative and enjoyable.

I. C. McILWAINE

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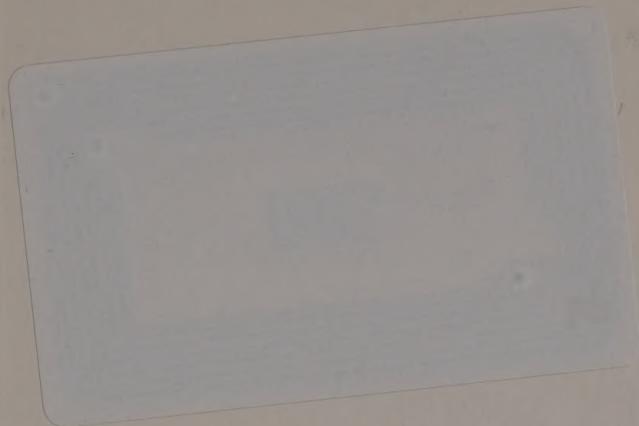
The following books have been received. The fact that they are listed here does not preclude their review in a later issue.

- ACTA INTERDISCIPLINARIA ARCHAEOLOGICA.* Tomus III. Nitra,
Archeologicky Ustav Slovenskej akademie vied, 1984. 331pp.
no price given.
- BLAZQUEZ MARTINEZ, J.M., GARCIA-GELABERT PEREZ, M.P. & PARDO, F.L.**
Castulo V. (Excavaciones arqueologicas en Espana) Madrid,
Ministerio de Cultura, 1985. 396pp. 164 figs. Price not
stated.
- BLAZQUEZ, J.M. & MEZQUIRIZ, M.A.** *Mosaicos Romanos de Navarra.* Madrid,
Instituto espanol de arqueologia del consejo superior de
investigaciones cientificas, 1985. 111p., 62 pls. Price not
stated
- BRUMFIELD, E.M. & EARLE, T.K.** *Specialization, exchange and complex
societies. (New directions in archaeology)* Cambridge: Cambridge
University Press, 1987. 150pp., illus. £25.00
- BULLETIN of the Anglo-Israel Archaeological Society, 1985-6.** London,
1986. 79pp., illus. Price not stated
- BURGOYNE, M.H.**, *Mamluk Jerusalem: an architectural study; with
additional historical research by D.S. Richards.* published on
behalf of the British School of Archaeology in Jerusalem by the
World of Islam Festival Trust, 1987. xii + 623pp., 64 figs.,
2 plans in slip case, 64 pls. +32 col. pls. Price not stated
- CLEERE, H. & CROSSLEY, D.** *The iron industry of the Weald.* Leicester
University Press, 1985. xvi + 395pp., 74 figs. £47.50
- CLOSE-BROOKS, J.** *Exploring Scotland's Heritage: The Highlands.*
Edinburgh, R.C.A.H.M.S./H.M.S.O., 1986. 184pp. Map. illus.
£6.95
- CONNAH, G.** *African civilizations: precolonial cities and states in
tropical Africa: an archaeological perspective.* Cambridge,
Cambridge University Press, 1987. xi + 259pp., illus. Hardback:
£25.00; paperback: £7.95.
- CRAWFORD, B.E.** *Scandinavian Scotland (Scotland in the early Middle
Ages, 2)* Leicester U.P., 1987. xii + 274pp. 80 figs. Hardback:
£30.00; paperback: £9.95.
- FARRINGTON, I.S.** *Prehistoric intensive agriculture in the tropics.*
(BAR International Ser., 232) 2 vols. Oxford, B.A.R., 1985. ix +
881pp., illus. £43.00
- FIEDEL, Stuart J.** *Prehistory of the Americas.* Cambridge, Cambridge
University Press, 1987. x + 386p., 113 figs. Hardback: £30.00;
paperback: £10.95
- GASULL, P., LULL, V. & SANAHUJA, M.A.** *Son Forques I: la fase Talayotica:
ensayo di reconstrucción socio-económica de una comunidad
prehistórica de la isla de Mallorca.* (BAR International ser., 209)
Oxford, BAR, 1984. 187pp. 65 figs. 21 pls. £14.00
- GODWIN, Sir H.** *History of the British flora: a factual basis of
phytogeography.* 2nd. ed. Cambridge: Cambridge University Press,
1984.

- GOING, C.J. *The mansio and other sites in the south-eastern sector of Caesaramagus: the Roman pottery; with contributions by J. Bayley (and others).* (Chelmsford Archaeological Trust report, 3.2) (CBA Research report, 62) London, published by the Chelmsford Archaeological Trust and the Council for British Archaeology, 1987. vii + 123pp. 59 figs. 2 pls. £16.95.
- HAAS, J., POZORSKI, S. & T. *The origins and development of the Andean State.* (New directions in archaeology) Cambridge: Cambridge University Press, 1987. vi + 188pp., illus. £30.00
- HADWICK, J. [et al]. *Corpus of Mycenaean inscriptions from Knossos.* Vol. 1: (1-1063). CUP/Ed. dell'Ateno, 1987. (Incunabula graeca, 88).
- HARDING, D.W. *The Iron Age in lowland Britain.* Routledge & Kegan Paul, 1985. Paperback ed. (First published 1974.) xviii + 260pp., 81 figs., 37 pls. £7.95.
- HAYDEN, B. *Lithic studies among the contemporary Highland Maya.* Tucson, The University of Arizona Press, 1987. xii + 387pp., illus. \$35.00
- HEGGIE, D.C. *Megalithic science: ancient mathematics and astronomy in N.W. Europe.* London: Thames & Hudson, 1981.
- HODDER, I. *Archaeology as long-term history.* (New directions in Archaeology). Cambridge, Cambridge University Press, 1987. vii + 145pp., illus. £25.00
- HUNTER-ANDERSON, R.L. *Prehistoric adaptation in the American Southwest.* (New studies in archaeology) Cambridge, Cambridge University Press, 1986. xiv + 143pp., 16 figs. 12 tabs. £22.00
- HURST, H.R. *Gloucester, the Roman and later defences: excavations on the E. Defences and a reassessment of the defensive sequence; with the assistance of L.F. Pitts.* (Gloucester archaeological reports, v.2) Gloucester, Gloucester Archaeological Publications, 1986. ix + 150pp., illus (figs & maps). £17.95.
- JACOBSON, J. *Studies in the archaeology of India and Pakistan.* Warminster, Aris & Philips in cooperation with the American Institute of Indian Studies, 1987; originally published in New Delhi by Oxford & I.B.H. Publishing co. xx + 327pp., illus. £16.00
- KOCH, U. *Die Metallfunde der frühgeschichtlichen Perioden aus den Plangrabungen, 1967-1981.* (Der Runde Berg bei Urach, bd 5) 2 vols. Heidelberg, Carl-Winter-Universitätsverlag, 1984. 272pp., illus.; 93 tabs., 47 fold. maps. Price not stated
- KSAR AKIL, LEBANON.
- AZOURY, I. *Ksar Akil, Lebanon: a technological and typological analysis of the transitional and early Upper Palaeolithic levels of Ksar Akil and Abu Halka.* Vol. 1, edited with an introduction by C. Bergman and L. Copeland. (BAR International ser., 289) 2 vols. Oxford, BAR, 1986. vii + 244pp., 202 figs., 122 pls. £30.00
- BERGMAN, C.A. *Ksar Akil, Lebanon: a technological and typological analysis of the later Palaeolithic levels of Ksar Akil.* Vol. 2: Levels xiii-vi, with contributions by L. Copeland and M. Newcomer. (BAR International ser., 329). Oxford, BAR, 1987. iii + 334pp., 87 figs. 2 plans, 29 pls. £20.00
- KEPPIE, L. *Scotland's Roman remains: an introduction and handbook.* Edinburgh, J. Donald, 1986. ix + 188pp. £7.50

- KIRCH, P.V. *Island societies: archaeological approaches to evolution and transformation*. (New directions in archaeology) Cambridge University Press, 1986. viii + 99pp., illus. £19.50.
- LIBBY, L.M. *Fast climates: tree thermometers, commodities and people*. Austin, University of Texas Press, 1983. xiv + 143pp., illus. \$25.00
- MACKENZIE, A. *Archaeology in Romania: the mystery of the Roman occupation*. Robert Hale, 1986. 183pp., 16pls. £14.95
- MATHIEN, F.J. & McGuire, R.H. *Ripples in the Chichimec Sea: new considerations of Southwestern-Mesoamerican interactions*. (Publications in archaeology) Carbondale & Edwardsville, Southern Carolina University Press, 1986. xvii + 300pp., 12 figs., 5 tbls. \$32.50
- MELICHER, J. *Bibliografic slovenskej archeológie za Rok 1981-82*. 2 parts. Nitra, Archeologicky Ustav Slovenskej Akademie vied, 1985. 76pp. Price not stated.
- MELICHER, J. *Bibliografia slovenskej archeológie za Rok, 1982*. Nitra, Archeologicky Ustav Slovenskej Akademie vied, 1985. 80pp. Price not stated
- MILISAUKAS, S. *Early Neolithic settlement and society at Olszanica*. (Memoirs of the Museum of Anthropology, University of Michigan, no. 19) Ann Arbor, University of Michigan, 1986. xx + 319pp., 160 figs., 153 tbls., 51 pls. \$20.00
- MILLETT, M. & GRAHAM, D. *Excavations on the Romano-British small town at Neatham, Hampshire, 1969-1979*. (Hampshire Field Club and Archaeological Society, monograph, 3) Gloucester, produced by Alan Sutton for the Hampshire Field Club in cooperation with the Farnham and District Museum Society, 1986. xvi + 166p., 104 figs., 33 tbls. £20.00
- MÖRNER, M. *The Andean past: land, societies and conflicts*. New York, Columbian University Press, 1985. xiii + 300pp., illus. \$39.00
- MORRISON, J.S. & COATES, J.F. *The Athenian trireme: the history and reconstruction of an ancient Greek warship*. Cambridge, Cambridge University Press, 1986. xxiii + 266pp., 75 figs., 15 maps. Hardback: £22.00; paperback: £7.50.
- NABHAN, G.P. *Gathering the desert*. Tucson, University of Arizona Press, 1985. ix + 209pp., illus. \$19.95
- NEEDHAM, J. *Science and civilisation in China. Vol 6: Biology and biological technology: pt.2: Agriculture*, by Francesca Bray. Cambridge, Cambridge University Press, 1984. xxvii + 724pp., 271 figs., 13 tbls. £50.00
- PARKER, S. T. *The Roman frontier in central Jordan: interim report on the Limes Arabicus project, 1980-85*. (BAR International ser, 340) 2 vols. Oxford, BAR, 1987. x + 428pp.; ii + 861pp., 150 figs., 109 pls. £50.00
- SANDERS, N.K. *Prehistoric art in Europe*. 2 ed. (Pelican History of Art) Harmondsworth, Penguin Books, 1985. 508pp., 442 illus., 3 maps. 3 tbs. £14.95.
- SCHIFFER, M.B. *Formation processes of the archaeological record*. Albuquerque, The University of New Mexico Press, 1987. xxix + 428pp., 70 figs. Hardback: \$39.95; paperback: \$19.95.

- SCHOFIELD, J. & LEECH, R. *Urban archaeology in Britain*. (CBA Research report, no 61). London, The Council for British Archaeology, 1987. x + 234pp., 103 figs., £19.50
- SHANKS, M. & TILLEY, C. *Reconstructing archaeology: theory and practice*. (New studies in archaeology). Cambridge, Cambridge University Press, 1987. xvi + 267pp., illus. £27.50.
- SHEPHERD, I.A.G. *Exploring Scotland's heritage: Grampian*. R.C.A.H.M.S./H.M.S.O., 1986. 184pp., map, illus. £6.95.
- SHIPLEY, G. *A history of Samos, 800-188 BC*. Oxford, Clarendon Press, 1987. xviii + 352pp., 24 figs., 16 pls. £35.00
- STELL, G. *Exploring Scotland's heritage: Dumfries and Galloway*. R.C.A.M.S./H.M.S.O., 1986. 184pp., map, illus. £6.95
- TERRELL, J. *Prehistory in the Pacific islands: a study of variation in language, customs and human biology*. Cambridge, Cambridge University Press, 1986. xvi + 299pp., 88 figs., 5 tabs. £30.00
- TOBIAS, Philip V. *Hominid evolution: past, present and future. Proceedings of the Taung Diamond Jubilee International Symposium, Johannesburg & Mbombela, Southern Africa, 27th Jan - 4th Feb 1985*. New York, A.R. Liss, 1985. xxix + 499pp., illus. £29.00
- VICKERS, M. *Pots & pans: a colloquium on precious metals and ceramics in the Muslim, Chinese and Graeco-Roman worlds*. Oxford, 1985. (Oxford studies in Islamic art, 3) Oxford, Oxford University Press for the Board of the Faculty of Oriental Studies, 1987. 223pp., illus. Hardback: £25.00; paperback: £15.00
- WAGSTAFF, J.M. *Landscape and culture: geographical and archaeological perspectives*. Oxford, B. Blackwell, 1987. 233pp., illus. Hardback: £29.50; paperback: £9.95
- WALKER, B. & RITCHIE, G. *Exploring Scotland's heritage: Fife and Tayside*. R.C.A.H.M/H.M.S.O., 1987. 202pp., map, illus. £6.95.
- WAYMENT, H. *The stained glass of the church of St Mary, Fairfield, Gloucestershire*. (Occasional papers (N.S.), the Society of Antiquaries of London, 5) London, Society of Antiquaries, 1984. xii + 115pp., 5 figs., 74pls. £15.00
- WILLEY, G.R. *Essays in Maya archaeology*. University of New Mexico, 1987. xix + 245pp., map. 6 pls. Hardback: \$27.50; paperback: 13.95.
- WILSON, D.M. *The forgotten collector: Augustus Wolleston Franks of the British Museum*. (16th Welton Newrath Memorial lecture) London, Thames & Hudson, 1985. 63pp., 39 pls. £4.50
- WOLFRAM, S. *Zur Theoriediskussion in der Praehistorischen Archaeologie Grossbritanniens: ein Forschungsgeschichtlicher Uberblick über die Jahr 1968-1982*. (BAR International Ser. 306) Oxford, BAR, 1986. 152pp., 12 figs. £10.00



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